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An Analysis of Unemployment II

By Sir William Beveridge

V

Duration of Unemployment

The Ministry of Labour publish each month a table headed "Periods of Registered Unemployment". In this table applicants for benefit or assistance are classified according to the length of time for which they have been registered continuously as unemployed. Occasional short spells of employment lasting not more than three days each are held not to break continuity of registration. Except in so far as they may have got work for such brief spells, the persons shown in these tables have been unemployed for the whole of the periods stated, namely for less than three months, less than six months, and so on, up to 12 months or more. These statistics have been published now for each month since the beginning of 1932; they are given separately for men from 18 to 64, for women from 18 to 64, for boys and for girls.

From two sample enquiries relating to 16th September, 1929, and 2nd February, 1931, it is possible to derive, by estimate, corresponding figures for men and women at those earlier dates.[1] It is possible also to obtain figures for each of the administrative Divisions, though these are not normally published by the Ministry.

In using these figures two points have to be borne in mind. First, they are based on counts at particular days;

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[1] The figures for 16th September, 1929, are published in the *Labour Gazette* for January, 1930, and those for 2nd February, 1931, in the Appendices to the Minutes of Evidence taken before the Royal Commission on Unemployment Insurance, Part V, p. 298. In each case the periods of registered unemployment are classified in a slightly different way from that of the current monthly statistics—by numbers of weeks or days and not by three months at a time. But the figures for those unemployed twelve months (52 weeks or 312 days) or more, are directly comparable throughout, and those recorded as unemployed up to 12 weeks in September, 1929, are also directly comparable with those unemployed less than three months in the current statistics, since broken weeks of unemployment are disregarded in the September, 1929, statistics. For the intervening Groups "3 months and less than 6 months", etc., interpolation has been necessary: thus, the numbers recorded as unemployed for 25 to 28 weeks have been treated as spread evenly over those four weeks.

they do not show how much unemployment has been suffered, in the course of a year or any other extended period, by those who become unemployed at any time during the period. All those who on any given day appear in the last section—as having been out for twelve months or more—will have appeared at an earlier date in the first section—as unemployed for less then three months—and will have progressed through the following sections. On the other hand, some of those who at any given date appear as having been out for less than three months may, within the past year, have had far more unemployment than employment.

Second, the figures relate only to applicants for benefit or assistance, and not to all the insured unemployed. The class of applicants is wider than Groups A and B in Table II. In addition to claims admitted for benefit or assistance, it includes claims under consideration and claims disallowed under the Means Test, disqualifications, or anomalies regulations, so long as the claimants continue to register; it excludes only the " two months " file and those who have not enough contributions to base a claim. Groups A and B of Table II cover about five out of every six of the recorded unemployed in the insured trades. The duration statistics, now under review, taking men and women together, cover seven out of eight. But the proportions covered for the two sexes are different : ten out of eleven among men and four out of five among women. This reflects the much larger proportions of women in the " two months " file or registering without claim to benefit or assistance (see Table XVI in next issue). In so far as persons excluded from the duration statistics are people on public assistance, the statistics understate the proportion of long-period unemployment. But this excluded long-period unemployment is largely that of persons whose contact with insured industry is slight.

For these two reasons, the duration statistics must be taken as a rough, rather than exact, indication of the distribution of unemployment among individuals and the proportions of long and short unemployment respectively. Nevertheless, the distinction between long and short unemployment in the individual case is of such importance, both theoretically and practically, that the figures deserve a good deal of attention.

It has not seemed worth while to reproduce here the figures published month by month in the *Labour Gazette* since January, 1932, but it may be of interest to draw attention to some of their features.

First, short unemployment (less than three months) has a well-marked seasonal fluctuation, with relatively high unemployment in January and in June and July. In the first months of the year and also from August to November, there is almost invariably a decline. There are indications of a seasonal movement, also, with later phases, in the figures relating to unemployment of three to six months, and of six to nine months. These variations reflect the net seasonal movement of employment and unemployment as a whole. So far as possible, in comparing one year with another, it is desirable to choose the same month in each year.

Second, short unemployment (less than three months), beginning at 1,086,539 for men and 232,950 for women in January, 1932, reached its peak alike for men (1,216,778) and for women (268,374) in August, 1932, which was also about the worst point of the industrial depression in Britain ; since then, till September, 1936, short unemployment has fallen by more than half, to 602,535 among men and by nearly a half—to 152,890—among women. Long unemployment among men (twelve months or more) from 300,336 in January, 1932, continued to rise till the middle of 1933 (459,192 in July), and since then has fallen proportionately much less than short unemployment to 306,014 in September, 1936. Among women, at the beginning of 1932, long unemployment was still in process of deflation by the Anomalies Act ; it has fallen almost without interruption from 36,784 in January, 1932, to 18,888 in September, 1936.

As has been stated, it is possible from sample enquiries to give by estimate comparable figures for the two earlier dates—16th September, 1929, and 2nd February, 1931. The first of these dates fortunately falls just before the beginning of the trade depression of 1930–32. The figures for February, 1931, show, not only the onset of the depression, but the relative inflation of women's unemployment by the easier conditions for benefit, after the abolition of the requirement that the applicant should be "genuinely seeking work", and before the introduction of the Anomalies Act.

Table IX gives for September, 1929, February, 1931,

Table IX

DURATION OF UNEMPLOYMENT AMONG APPLICANTS FOR BENEFIT OR ALLOWANCE AT CERTAIN DATES (GREAT BRITAIN)

	Last Period of Registered Unemployment						
Date	Less than 3 months	3 months and less than 6	6 months and less than 9	9 months and less than 12	12 months or more	All Applicants	All Insured Unemployed[1]
				Men aged 18–64			
September, 1929	606,200	83,950	33,100	19,850	43,000	786,100	914,246
February, 1931	1,101,200	320,600	167,000	102,600	89,400	1,780,800	1,918,093
August, 1932	1,216,778	244,744	168,545	147,064	383,838	2,160,969	2,329,471
August, 1933	943,641	193,924	128,919	108,274	457,060	1,831,818	2,003,281
August, 1934	839,336	144,459	97,592	76,039	376,826	1,534,252	1,697,161
August, 1935	722,529	132,553	94,502	74,021	359,797	1,383,402	1,535,986
August, 1936	579,297	105,550	70,555	54,763	312,635	1,122,800	1,245,537
September, 1936	602,535	104,798	69,114	51,505	306,014	1,133,966	1,250,783
				Women aged 18–64			
September, 1929	152,600	18,950	4,150	2,900	2,100	180,700	218,009
February, 1931	334,800	110,800	60,200	35,600	28,600	570,000	610,980
August, 1932	268,374	33,039	15,973	9,379	28,407	355,172	451,548
August, 1933	184,600	28,130	14,007	6,808	21,094	254,639	334,110
August, 1934	216,699	23,126	11,504	5,837	16,761	273,927	332,448
August, 1935	177,513	22,207	12,178	6,773	19,154	237,825	295,088
August, 1936	148,566	19,757	9,994	5,456	19,000	202,773	258,021
September, 1936	152,890	18,827	9,619	5,002	18,888	205,226	258,583
				Men and Women aged 18–64			
September, 1929	758,800	102,900	37,250	22,750	45,100	966,800	1,132,255
February, 1931	1,436,000	431,400	227,200	138,200	118,000	2,350,800	2,529,073
August, 1932	1,485,152	277,783	184,518	156,443	412,245	2,516,141	2,781,019
August, 1933	1,128,241	222,054	142,926	115,082	478,154	2,086,457	2,337,391
August, 1934	1,056,035	167,585	109,096	81,876	393,587	1,808,179	2,029,609
August, 1935	900,042	154,760	106,680	80,794	378,951	1,621,227	1,831,074
August, 1936	727,863	125,307	80,549	60,219	331,635	1,325,573	1,503,558
September, 1936	755,425	123,625	78,733	56,507	324,902	1,339,192	1,509,366

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[1] Excluding Social Schemes for Banking and Insurance.

August, 1932 (the bottom of the trade depression), August in each year 1933 to 1936, and September, 1936, the numbers unemployed for various lengths of time: men and women are shown separately and together. Table X presents these numbers as percentages of the totals unemployed at those dates. The choice of the August or September in each year except 1931 avoids disturbance of comparisons by seasonal fluctuation. The tables, like the comments above, are limited to men and women of 18 to 64; there is little prolonged unemployment among boys and girls of 16 or 17.

The main interest of the tables lies, as has been said, in the comparison of September, 1929, before the depression, with September, 1936, as the most recent time of activity after depression. Looking first at the actual numbers, it is seen that the total of unemployed applicants in September, 1936, is about 372,000 more than in September, 1929. 75 per cent. of this addition is represented by persons who have been unemployed continuously for twelve months or more—nearly the whole of the remaining addition represents persons who have been unemployed between six months and twelve months. The number of applicants unemployed for less than three months is below the level of 1929, while the number of those unemployed from three to six months is slightly higher.

It has to be remembered, however, that between the dates compared, the total number of insured persons has risen by more than 10 per cent. In September, 1929, the unemployed percentage of Britain was 9.8, practically at the same level (apart from the dispute year, 1926) as it had maintained since the end of the previous depression in 1923. In September, 1936, the unemployed percentage was 11.9. The whole increase of 2.1, representing about 275,000 persons, is less than the addition to those unemployed for twelve months or more. Allowing for the increase in the numbers insured, it is fair to say that in September, 1936, there was appreciably less unemployment up to six months than in September, 1929. There was six-and-a-half times as much long unemployment, i.e. unemployment of twelve months or more.

Table X illustrates the same fact in another way. In September, 1929, nearly 90 per cent. of the applicants had been out of work for less than six months, and less than

TABLE X

DURATION OF UNEMPLOYMENT: PERSONS UNEMPLOYED FOR VARIOUS LENGTHS OF TIME AS PERCENTAGES OF ALL APPLICANTS FOR BENEFIT OR ALLOWANCE (GREAT BRITAIN)

Date	Last Period of Registered Unemployment						
	Less than 3 months	3 months and less than 6	6 months and less than 9	9 months and less than 12	12 months and more	All Applicants	All Insured Unemployed[1]
				Men aged 18–64			
September, 1929	77.1	10.7	4.2	2.5	5.5	100.0	116.3
February, 1931	61.8	18.0	9.4	5.8	5.0	100.0	107.7
August, 1932	56.3	11.3	7.8	6.8	17.8	100.0	107.8
August, 1933	51.5	10.6	7.0	5.9	25.0	100.0	109.4
August, 1934	54.7	9.4	6.4	5.0	24.5	100.0	110.6
August, 1935	52.2	9.6	6.8	5.4	26.0	100.0	111.0
August, 1936	51.6	9.4	6.3	4.9	27.8	100.0	110.9
September, 1936	53.2	9.2	6.1	4.5	27.0	100.0	110.3
				Women aged 18–64			
September, 1929	84.4	10.5	2.3	1.6	1.2	100.0	120.6
February, 1931	58.7	19.4	10.6	6.3	5.0	100.0	107.2
August, 1932	75.6	9.3	4.5	2.6	8.0	100.0	127.1
August, 1933	72.5	11.0	5.5	2.7	8.3	100.0	131.2
August, 1934	79.1	8.5	4.2	2.1	6.1	100.0	121.4
August, 1935	74.6	9.3	5.1	2.9	8.1	100.0	124.1
August, 1936	73.3	9.7	4.9	2.7	9.4	100.0	127.2
September, 1936	74.5	9.2	4.7	2.4	9.2	100.0	126.0
				Men and Women aged 18–64			
September, 1929	78.5	10.6	3.8	2.4	4.7	100.0	117.1
February, 1931	61.1	18.3	9.7	5.9	5.0	100.0	107.6
August, 1932	59.0	11.1	7.3	6.2	16.4	100.0	110.5
August, 1933	54.1	10.6	6.9	5.5	22.9	100.0	112.0
August, 1934	58.4	9.3	6.0	4.5	21.8	100.0	112.2
August, 1935	55.5	9.5	6.6	5.0	23.4	100.0	112.9
August, 1936	54.9	9.5	6.1	4.5	25.0	100.0	113.4
September, 1936	56.4	9.2	5.9	4.2	24.3	100.0	112.7

[1] Excluding Social Schemes for Banking and Insurance.

5 per cent. had been out for twelve months or more. In September, 1936, these proportions have become 66 per cent. and 24 per cent. respectively. The legacy of the great depression is a mass of long-period unemployment—more than 350,000 men and women in insured trades, and for whose case continuous money payments in idleness are a manifestly inadequate provision.

The sample enquiry of September, 1929, dealt also with the age distribution of applicants unemployed for various periods. Table XI shows for men unemployed twelve months or more the percentage in various age groups, in September, 1929, and in July, 1935. It will be seen that the much greater number at the later date includes a larger proportion of men under 45. In 1935, there were nearly as many men 18–24 unemployed for twelve or more months, as there were men of all ages in like condition in 1929.

TABLE XI

AGE DISTRIBUTION OF APPLICANTS UNEMPLOYED 12 MONTHS OR MORE. (Men 18–64)

Age	September, 1929		July, 1935	
	Numbers	Percentage	Numbers	Percentage
18–24	2,800	6.5	38,030	10.5
25–34	8,000	18.6	78,638	21.7
35–44	8,100	18.9	72,691	20.1
45–54	10,700	24.9	78,024	21.6
55–59	6,500	15.1	48,071	13.3
60–64	6,900	16.0	46,123	12.8
	43,000	100.0	361,577	100.0

Another matter of interest in regard to long-period unemployment is its local distribution. This is shown in Table XII for January, 1932, and for June in four years thereafter. There are marked differences between different parts of the country. In London and the South-East of England unemployment for twelve months and over, among men applying in June, 1936, is only 9 per cent. of the total unemployment, and in the South-Western Division is 14 per cent. In the Midlands, the North-East and North-West it ranges from 25 per cent. to 27 per cent. In Scotland, men who have been unemployed for twelve months and over are 30 per cent. of all the applicants, and in Wales

TABLE XII

UNEMPLOYED APPLICANTS FOR BENEFIT OR ALLOWANCE AT CERTAIN DATES—BY PERIOD OF UNEMPLOYMENT AND DIVISION. (Men 18–64)

Division and Insured Men, July, 1936	Date	Period of Registered Unemployment					
		Less than 3 months	3 months and less than 6	6 months and less than 9	9 months and less than 12	12 months or more	Total
LONDON (1,649,460)	26.1.32	157,825	49,177	18,700	10,122	9,626	245,450
	27.6.32	144,523	36,834	19,698	12,803	9,902	223,760
	26.6.33	119,391	30,530	16,321	10.415	13,174	189,831
	25.6.34	95,272	18,208	10,128	6,029	8,418	138,055
	22.6.36	77,105	12,689	7,322	4,469	9,306	110,891
SOUTH EAST (812,990)	26.1.32	83,779	24,612	7,213	3,305	3,778	122,687
	27.6.32	63,997	14,867	8,687	4,437	3,624	95,612
	26.6.33	47,129	9,623	5,224	3,485	4,899	70,360
	25.6.34	34,856	5,834	3,455	1,912	3,157	49,214
	22.6.36	29,762	5,008	3,216	1,902	3,697	43,585
SOUTH WEST (705,140)	26.1.32	79,817	25,836	8,474	4,683	8,606	127,416
	27.6.32	71,657	16,135	10,053	6,914	10,060	114,819
	26.6.33	63,457	14,104	8,067	5,587	13,129	104,434
	25.6.34	53,125	9,500	5,913	3,815	10,070	82,423
	22.6.36	37,219	6,433	3,520	2,523	8,149	57,844
MIDLANDS (1,350,940)	26.1.32	150,635	42,062	24,075	18,464	38,516	273,752
	27.6.32	181,210	33,907	25,577	21,043	44,772	306,509
	26.6.33	139,238	29,521	20,414	16,066	56,068	261,307
	25.6.34	120,099	18,322	12,547	9,502	40,344	200,814
	22.6.36	72,982	13,378	8,692	6,182	30,433	131,667
NORTH EAST (1,536,010)	26.1.32	205,535	68,755	44,709	33,740	85,392	438,131
	27.6.32	261,548	51,296	42,521	40,530	105,494	501,389
	26.6.33	203,455	44,125	32,600	31,114	141,922	453,216
	25.6.34	201,552	33,334	23,715	18,855	119,436	396,892
	22.6.36	166,502	24,793	17,868	14,350	89,822	313,335

TABLE XII (*continued*)

Division and Insured Men, July, 1936	Date	Period of Registered Unemployment: Less than 3 months	3 months and less than 6	6 months and less than 9	9 months and less than 12	12 months or more	Total
NORTH WEST (1,372,840)	26.1.32	192,590	55,298	35,098	25,192	63,295	371,473
	27.6.32	209,026	39,272	30,051	27,468	68,280	374,097
	26.6.33	171,537	38,223	27,456	22,676	82,802	342,694
	25.6.34	152,702	29,939	20,002	16,250	70,873	289,766
	22.6.36	117,097	25,306	17,155	12,396	64,215	236,169
SCOTLAND (935,650)	26.1.32	118,689	46,285	28,538	21,597	57,434	272,543
	27.6.32	118,794	30,858	27,800	23,166	76,425	277,043
	26.6.33	95,091	28,105	21,714	19,169	83,735	247,814
	25.6.34	81,842	23,362	18,107	14,146	71,136	208,593
	22.6.36	63,581	19,069	12,834	9,738	55,572	160,794
WALES (512,790)	26.1.32	97,669	26,859	17,971	14,033	33,689	190,221
	27.6.32	114,225	20,896	16,556	14,409	44,362	210,448
	26.6.33	87,671	15,448	14,382	14,110	62,399	194,010
	25.6.34	86,204	15,458	12,379	11,635	64,348	190,024
	22.6.36	69,684	14,225	11,402	10,104	63,744	169,159
BRITAIN (8,876,000)	26.1.32	1,086,539	338,884	184,778	131,136	300,336	2,041,673
	27.6.32	1,164,980	244,065	180,943	150,770	362,919	2,103,677
	26.6.33	927,059	209,679	146,178	122,622	458,128	1,863,666
	25.6.34	825,652	153,957	106,246	82,144	387,782	1,555,781
	22.6.36	633,932	120,901	82,009	61,664	324,938	1,223,444

37 per cent. To the total of 325,000 men unemployed in June, 1936, who had been out for twelve months or more, London, the South-East and South-West contribute only 21,000; the North-Eastern Division alone has nearly 90,000 and Wales nearly 64,000.

In most divisions, long-period unemployment, having risen from the earliest recorded figures at the beginning of 1932, to a maximum in or near the middle of 1933, has fallen again, and is now back at about the same level as at the beginning of 1932. But to this statement Wales presents a solitary and tragic exception. In Wales, there were in June, 1936, nearly twice as many men of long unemployment as there were at the beginning of 1932—there were more than at the depth of the depression and in the worst times of 1932 or 1933. The cloud of chronic unemployment in Wales has not lifted at all.

The distinction made here between long and short period unemployment is of great practical importance. Short-period unemployment—anything up to three months or even up to six months can be, and on the whole is, dealt with efficiently by unemployment insurance. Under the present scheme, any man who at the beginning of the benefit year can prove his claim to benefit gets benefit for at least six months, if he remains unemployed so long. If he has a good record of unemployment, sufficient to qualify him for additional days under the Act of 1934, he can continue on benefit even longer; 10 per cent. of the men in receipt of benefit at June, 1936, had been unemployed continuously for more than six months, and 3 per cent. for twelve months or more. The bulk of those who are recorded as unemployed for less than six months are, in fact, covered by the insurance scheme. They get benefit not reduced by their possession of other means, nor by the fact that they can be helped by some of their family. They can carry through without deterioration, till they can get work again. Those who have been unemployed longer—for nine or twelve months or more—are an altogether different problem. They need, if anything, more money rather than less than those who have had short periods of unemployment. Yet they can hardly be given more money, without being encouraged to settle down into permanent unemployment. Nor is it possible for most of them to escape physical and psychological deterioration through long idleness.

Long-period unemployment is the most serious element in the problem of unemployment and to-day forms a larger proportion of the total problem than it used to do.

VI

Age and Unemployment

The influence of age upon unemployment is important, but does not show itself in the way that is commonly supposed. In discussing this question it is necessary to begin by disposing of two popular fallacies.

In the first place, the assertion is often made, as if it were axiomatic, that the "speeding up" of industry is driving men into retirement at an earlier age than before: that from being too old at sixty, men are coming to be too old at fifty-five or earlier. So far as I know, there is no evidence for this assertion. The only statistical evidence known to me—derived from the age of superannuation in certain large trade unions—tells in the opposite direction, and suggests that, both before and since the war, the age of retirement from industry has been rising and not falling.[1] This evidence is too limited to be conclusive. But on general grounds it can be suggested that there is no reason for assuming that technical progress puts a premium on youth; it may just as well work in favour of age, by making sheer physical vigour less important. The general improvement of health which is lengthening the total life of men must also, to some extent, be helping to maintain till later their working capacity.

In the second place, it is assumed by many people that an older man is more likely to lose his job than a young one. This is definitely not the case. The risk of an employed man losing his employment is practically the same from 55 to 64 as from 45 to 54 or 35 to 44, and is actually less than in the decade 25–34. This is shown by two independent sample investigations of the insured population made by the Ministry of Labour and relating to December, 1930, and December, 1932, respectively.[2] The results of these investigations, on this point, are set out in

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[1] Beveridge, *Unemployment: A Problem of Industry* (1930 Edition), pp. 121–123, 443–446.

[2] The results of the first investigation were published in Part V of Appendices to the Minutes of Evidence taken before The Royal Commission on Unemployment Insurance, and that of the second in the *Ministry of Labour Gazette*, September and October, 1933.

Table XIII. The upper half of the table, relating to the earlier sample, shows that the proportion of men in the age group 55–64 who made no claim to benefit in just over seven years from October, 1923, to the end of 1930 was 38.3 per cent. of all men at those ages. The corresponding percentages for ages 45–54 and 35–44 are 39.1 and 37.6—that is practically the same. The percentage for the age-group 25–34 was lower (31.2); that is to say, the proportion of men in this group who made at least one claim in the period is higher; the chance of falling out of employment is greater from 25 to 34 than it is from 35 to 64. The lower half of the table, relating to the later sample, substantially repeats this result, though the proportions not claiming at all are lower in all age-groups: this is natural, since the total period covered by the enquiry is longer and includes two years of depression in 1931 and 1932. These tables confirm conclusions reached as the result of earlier enquiries covering shorter periods.[1] The close agreement of the two independent samples in Table XIII shows the value of the sampling method.

But though, in the insured industries as a whole, the risk of losing employment is much the same at all ages from 35 to 64, the risk of being unemployed is not the same. Proportionately to the numbers in each age-group there are at any time more men unemployed between 45

TABLE XIII

CLAIMS TO BENEFIT BY AGE-GROUPS FOR MALES 16—64

Sample of December, 1930 (Claims since 18th October, 1923)

Number of Claims Made	Percentage in each age group					
	16—24	25—34	35—44	45—54	55—64	All Ages
0	52.7	31.2	37.6	39.1	38.3	41.1
1	20.7	18.1	16.3	15.2	13.6	17.7
2	11.0	14.0	11.8	11.3	10.7	11.9
3	7.4	11.2	9.8	9.7	9.8	9.4
4	4.5	9.8	8.6	8.5	9.1	7.6
5	2.4	7.4	7.1	7.2	7.6	5.7
6	1.0	5.1	5.0	4.7	5.3	3.7
7	0.3	2.6	2.8	3.2	4.1	2.2
8	0.0	0.6	1.0	2.1	1.5	0.7

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[1] See *Unemployment: A Problem of Industry* (1930 Edition), pp. 440–442, for results of earlier enquiries—in 1895 as to certain branches of the Amalgamated Society of Engineers; in 1927 as to the whole insured population.

Sample of December, 1932 (Claims since 7th July, 1924)

Number of Claims Made	Percentage in each age group					Total
	Under 25	25—34	35—44	45—54	55—64	
0	39.3	27.5	33.4	33.3	32.9	33.0
1	18.7	14.8	12.9	13.5	10.6	14.6
2	16.3	11.9	10.3	9.3	9.7	11.9
3	11.9	11.5	9.5	9.5	9.6	10.6
4	6.5	9.4	8.7	8.3	7.9	8.2
5	3.9	8.7	8.5	8.2	8.1	7.4
6	2.3	7.0	7.0	6.9	7.9	6.0
7	0.9	5.6	5.4	6.0	6.5	4.7
8	0.2	3.1	3.5	4.0	5.4	3.0
9	—	0.5	0.8	1.0	1.4	0.6
Total	100.0	100.0	100.0	100.0	100.0	100.0

and 54 than between 35 and 44, and still more between 55 and 64.

The growing incidence of unemployment with age is shown sufficiently by columns 4, 5 and 6 of Table XIV below, giving the unemployment percentage for men in each age-group from 18 to 64 at three particular dates—April, 1927, February, 1931, and November, 1932. The first of these dates is one of relatively good employment; the other dates fall in the great depression. In each case unemployment in the group 60–64 is about 50 per cent. above the average for men of all ages, and in the group 55–59 is 25 per cent. or more above the average.

The risk of losing one's job does not increase with advancing years. The risk of being out of a job does increase materially. The explanation is obvious—that the older man, once he has lost a job, finds it harder than a young man to get a new job. The older man has less power of recovery industrially, from loss of employment, as he has less power of recovery physically, from sickness or accident. Once he has become unemployed, he is likely to remain unemployed for longer than a young man.

This can be shown directly from the records of those who are unemployed for long periods. The Ministry of Labour make a classification of those who register as applicants for benefit or assistance each month, according to the length of time for which they have been unemployed continuously. Last year they made a special study of those who on 29th July, 1935, had been unemployed for twelve months or more, analysing them by industries and

TABLE XIV

UNEMPLOYMENT IN AGE GROUPS: MALES 18–64

Age Group	Estimated Numbers Insured (thousands)		Percentages Unemployed			Unemployed Nov., 1932		Unemployed 12 months or more at July, 1935		
	July, 1932	July, 1935[1]	April, 1927	Feb., 1931	Nov., 1932	Number (thousands)	Percentage of all Unemployed	Number	Percentage of all Unemployed	Percentage of Insured Persons
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
18–20 ..	896	926	8.8	15.8	16.3	145.8	7.5	6,294	1.7	.7
21–24 ..	1,071	1,104		23.3	23.5	251.7	12.9	31,736	8.8	2.9
25–29 ..	1,293	1,338	11.0	21.3	22.7	294.0	15.1	78,638	21.7	3.1
30–34 ..	1,147	1,188	9.3	22.0	21.9	250.8	12.9			
35–39 ..	876	907	9.2	22.3	21.4	187.8	9.6	72.691	20.1	4.2
40–44 ..	812	842	9.5	22.3	22.4	181.5	9.3			
45–49 ..	711	739	10.8	24.4	23.1	164.1	8.4	78,024	21.6	5.4
50–54 ..	678	702	11.3	27.1	26.5	180.0	9.3			
55–59 ..	576	599	13.0	28.5	26.9	154.8	8.0	48,071	13.3	8.0
60–64 ..	428	440	15.1	34.5	32.0	137.1	7.0	46,123	12.8	10.5
18–64 ..	8,488	8,785	10.2	23.1	22.9	1947.6	100.0	361,577	100.0	4.1

[1] Age distribution of insured males assumed same as in July, 1932.

ages. The last column of Table XIV shows with what crushing weight prolonged unemployment—the worst unemployment of all—falls upon the older men. The percentage of all insured men of 60–64 who at July, 1935, had been unemployed for twelve months or more was 10.5 per cent. as compared with 3.1 per cent. for men between 25 and 34 and 4.2 per cent. between 35 and 44.

In terms of risk the position can be summed up as follows. The risk of losing employment is much the same from 60 to 64[1] as from 35 to 44. The risk of being unemployed is half as much again at the later age than at the earlier. The risk of unemployment prolonged for a year or more is two and a half times as great.

In terms of insurance premiums this may be translated:—On a commercial basis, the premium for a policy covering all days of unemployment would be half as much again for a man in the age-group 60–64, as for one in the age-group 35–44. If the policy had to cover the first day of unemployment only, the premium in these two age-groups would be the same, and less than the premium from 25 to 34. If, on the other hand, the insured person carried the first twelve months of unemployment, and the policy covered only unemployment beyond that time, the premium for the older men would be two and a half or three times that required of the younger men.

The relation between age and prolonged unemployment is shown in another way in Table XV, where those unemployed for one year or more are subdivided according as their unemployment has lasted one year but less than two years, two years but less than three years, and so on up to five years or more. It will be seen that the later age-groups contribute steadily increasing proportions, as unemployment lengthens to become chronic. 64 per cent. of those unemployed for five years or more are aged 45 or over, but only 40 per cent. of those unemployed for between one and two years. The table, like others showing duration of unemployment, is limited to men who are claiming for benefit or assistance, that is to say to men who have had work in the past to justify their ranking as men normally employed in an insured trade.

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[1] The figures in Table XIII relate to the age group 55–64 and separate figures are not given for 60–64. But except on the extremely improbable supposition that the risk is materially less from 55 to 59 than at any earlier age, the risk from 60 to 64 cannot be greater.

TABLE XV

MEN UNEMPLOYED FOR TWELVE MONTHS OR MORE AT JULY, 1935

Percentages in Various Age Groups

	Period of registered unemployment					
Percentages in age-groups	1 year but < 2 years	2 years but < 3 years	3 years but < 4 years	4 years but < 5 years	5 years or more	Total 1 year or more
18–20	3.4	1.7	.5	.2	—	1.8
21–24	11.1	9.6	8.1	5.6	3.4	8.8
25–34	24.0	22.3	21.6	19.4	15.7	21.7
35–44	20.2	20.5	20.6	20.5	17.0	20.1
45–54	19.6	21.2	22.1	23.7	26.0	21.6
55–59	11.4	12.7	13.8	15.6	17.8	13.3
60–64	10.3	12.0	13.3	15.0	20.1	12.7
	100.0	100.0	100.0	100.0	100.0	100.0
Total number ..	134,035	78,808	63,664	52,570	32,500	361,577

It will have been noted that all the figures given above relate only to men. The corresponding statistics for women have not yet been examined. The interesting question, therefore, of whether age affects in the same way or in materially different ways the incidence of unemployment on the two sexes remains for the present unanswered.

More than 27 years ago I wrote that "the adverse influence of advancing years is . . . . . seen less when it is a question of retaining old employment than when it is a question of finding new employers".[1] I urged as one of the real remedies for unemployment by organisation an attempt to facilitate the re-employment of these older men. "In any large single undertaking, e.g. a railway company, there is always a certain number of old men's places—light situations—kept for those who have grown grey or become injured in the company's service. In industry as a whole there are no doubt also a good many such places, yet nothing to keep them for those whom they best fit. Work which older men could do, now, perhaps, by the chances of the labour market, falls to younger men and wastes their youthful vigour and adaptability. A Labour Exchange backed by sympathetic public opinion might do much to get all the old men's places for the older men and leave to the younger generations the task of finding and forcing fresh openings for themselves. It should be noted that, though 'old' men's places are spoken of, the actual men would often be at most middle-aged and with a long character to back them might be just those whom an employer would in any case choose if he got to know them."[1]

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[1] *Unemployment: A Problem of Industry*, 1909 Edition, p. 121.

When I wrote these words, the statement as to the effect of age on unemployment was little more than an impression derived from personal dealings with numbers of unemployed men. Since then the diagnosis has been confirmed repeatedly by statistical enquiries, covering now the whole insured population. The remedy remains to be applied. The perpetual favouring of younger men merely for their youth, in filling jobs within the competence of older men, makes for unemployment. It would not happen in a socialist community with employment unified. In an individualist community it is an anti-social act and should be recognised as such.

(*The third and final part of this article, examining Unemployment by sex, with an extended summary of conclusions, will appear in the May issue.*)

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[1] *Op. cit.*, p. 211.

The Working of the Pre-War Gold Standard

By P. B. Whale

I

In discussions concerning the post-war gold standard and the possibility of restoring an efficient international monetary system, it is natural to take the operation of the gold standard in pre-war times as a kind of standard. It is therefore of more than historical interest to know exactly how the pre-war gold standard did work.

The most generally accepted view on this matter—the classical explanation, as it may be conveniently termed—may be summarised as follows. A condition of balance of payments disequilibrium led, if more than temporary, to an international gold movement: the gold movement induced changes in the volume of monetary circulation in the countries concerned, a contraction here and an expansion there: the changes in circulation brought about the changes in incomes and prices required to adjust the balance of trade. In establishing a connexion between the gold movement and the change in the volume of circulation, it is necessary to take account of the nature of the currency system involved. As credit means of payment, bank liabilities in the form of notes and deposits, have long been the most important forms of money in the more advanced countries, the effect of a gold movement there depended on the reaction of the banking system to a change in its reserves; and in so far as a given change in reserves called for a *multiple* change in the bank liabilities, the process normally involved a change in the terms on which the banks lent, or the discount rate.

We shall not be concerned here to discuss the part played by price and income changes in maintaining international equilibrium. Our concern is with the process by which these price and income changes were brought about. As there appears to be some difference of opinion as to which points in the foregoing explanation are really important, it may be well to state that in this discussion the two following will be regarded as crucial :—

(1) The view that general changes in incomes and prices only occur as a result of changes in the volume of circulation. This may be termed the Quantity Theory assumption. (It will be noticed that if the Quantity Theory means this, it is not a truism.)

(2) The view that in a modern economy these changes in circulation are brought about by changes in discount or interest rates. This means of course that the maintenance of the gold standard requires from time to time a disturbance in the processes of saving and investing in particular countries : if true, it provides an important argument against the gold standard system.[1]

It may be noticed that I do not regard the part played by actual gold movements as crucial. Already, before the War, foreign balances were coming to be used as reserves in place of gold, and some writers make a great deal of this change. It may certainly have had the effect of making adjustments more " one-sided ", but apart from this, it does not appear to me to necessitate any important modification of the theory, at any rate so long as the exchange reserves are treated in the same way as gold reserves. I shall however have something to say about the practice of basing money on foreign balances in one particular connexion (see V below).

II

It is noteworthy that Taussig, although in a way he has been the leading exponent of the classical explanation, has often warned his readers that the evidence gives the theory very doubtful support. Further, I think it may be said that on the whole the attempts at historical verification undertaken by the younger members of the Harvard School have increased rather than diminished the doubts. (This is least true perhaps of Professor Viner's book, but Viner does not share my view as to which points are " crucial ".)

In fairness certain circumstances may be mentioned which would tend in any case to make verification difficult.

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[1] Cf. C. H. Walker, "The Working of the Pre-War Gold Standard," *Review of Economic Studies*, Vol. I, No. 3.

(1) The distribution of newly mined gold obviated to a certain extent the necessity for gold movements between non-producing countries. A country could suffer a *relative* loss of gold through failing to acquire its normal share in the new supplies.

(2) Similarly, in the adjustment of price relations between two countries, it would not be necessary for their discount rates actually to move in *opposite* directions, if there were at the same time a general upward or downward trend in rates.

But when account has been taken of these, certain difficulties remain.

(1) The point which has troubled Taussig most is that in many cases the adjustments appeared to have occurred more immediately and with less friction than his theory would lead one to expect.

(2) The dependence of international price adjustments on national discount policies raises the question whether discrepancies in interest rates might not lead to inconvenient capital movements. This has often been discussed as a theoretical issue. The evidence collected by Professors Andrew and Beach, to which further reference will be made presently, appears to show that in fact gold movements to and from both the United States and Britain before the War are more easily connected with relations of interest rates than with relations of price-levels.

(3) The pre-War policy of certain leading central banks hardly seems to have been conformable to the requirements of a gold standard operating in the manner envisaged by the classical explanation. It is notorious that both the Bank of France and the National Bank of Belgium consistently pursued the policy of keeping their discount rates low and steady.[1] It may be said that the Bank of France was able to avoid varying its discount rates because the country was only on a limping standard. But this point cannot be substantiated unless it can be shown that the exchange value of the franc fell below the gold export point for appreciable periods. Other special circumstances might be adduced in the case of France (*e.g.* the limited importance of credit money, in later years the size of the metallic reserve). But it is doubtful if they would apply to Belgium also, and the fact that both these countries were able to

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[1] Except in the case of the former for a period in the 'fifties and 'sixties.

maintain their exchange parities through changing circumstances, while their discount rates only followed the movements of rates elsewhere in a diminished degree, is certainly remarkable. The case of the Bank of England is different, for that institution certainly did have to take active measures, including the variation of its discount rate, for the protection of its gold reserve. But the object of discount policy may be to control capital movements, and the recent studies of Mr. Sayers and Professor Beach show that the Bank often preferred other devices to the use of its discount rate in order to avoid disturbing internal credit conditions. In fact the Bank seems to have been reluctant to raise the discount rate unless internal trade could stand it or actually needed a curb (*i.e.* from the point of view of internal stability). In short, then, none of these central banks really observed " the rules of the game " ; and yet the system worked.

(4) It is sometimes asked why a different process of adjustment should be necessary in the relations between different countries from that which may be supposed to operate in the relations between different parts of the same country. The fairly obvious answer is that within any country, at any rate if it has a unitary banking system, the common money in the hands of the public can be shifted from one part to another, whereas between countries direct monetary transfers mean movements of the *reserves* on which a multiple superstructure of credit is based. But on this view Scotland with its separate banking system should be related to England as a foreign country rather than as a different part of Britain. Admittedly the reserves of the Scottish banks take the form of London balances rather than gold holdings. But is there evidence that price equilibrium between England and Scotland is maintained by the Scottish banks varying their interest rates in response to changes in their reserves ?

These are my main grounds for doubts, but two other cases may be mentioned which, although they do not belong to the pre-War working of the gold standard, none the less serve to test the theory underlying the classical view on that matter.

(5) I am told, by Mr. Ashton, that serious difficulties were often experienced in Lancashire in the late 18th century owing to the shortage of local means of payment, and that

various expedients had to be resorted to in consequence. Now according to orthodox theory the local price level should have adjusted itself to the actual supply of means of payment ; and if this meant that the price level was unduly low in relation to other parts of the country, the favourable balance of trade should have brought in supplies of money to raise the price level. But this did not happen apparently ; the local price level was determined more directly by the national system of prices. Might not the national price level be similarly determined by the world system of prices ?

(6) The fall of prices in France in 1920, when the franc was still inconvertible, presents an interesting case. In the spring of that year certain measures of a deflationary tendency were adopted. The discount rate of the Bank of France was raised ; taxation was increased to improve the budgetary position ; and provision was made for annual reductions of the indebtedness of the government to the central bank. I do not think it can be denied that there was a connexion between these measures and the ensuing fall in prices. It is possible to argue that the fall was simply the result of internal deflation. On the other hand, having regard to the magnitude of the effect and the comparative inefficacy of similar deflationary measures at other times, it may be more plausible to connect the fall in French prices with the fall in world gold prices which took place at the same time. On this view the significance of the French measures was that by restoring confidence in the currency for the time they prevented the exchange from depreciating ; with the exchanges comparatively steady, the French price system became subject to world influences. This at any rate is the interpretation favoured by most French economists.

III

The crucial points in the classical explanation were challenged long ago by the writers of the Banking School. But the members of this school were unable to offer a satisfactory alternative explanation ; and it is mainly the absence of this, I think, which accounts for the continued predominance of the classical view. The purpose of what follows is to provide an alternative hypothesis.

We may begin with a line of thought first developed, I believe, by Mr. Harrod in his *International Economics*.[1] Mr. Harrod points out that a decline in the effective demand for a country's exports automatically curtails the incomes of the exporters, and through these, the incomes of those who sell to the exporters, and so on. Further it is shown that, in the absence of a counteracting influence, the contraction of incomes must proceed just far enough to restore the balance of trade; for the contracting tendency is only exhausted by people being induced to economise on foreign trade goods (imports or home produced goods which can find an export market). A counteracting influence is present, however, in the fact that individuals will reduce their money balances, either because they wish for a time to maintain their expenditure despite the reduction of their incomes, or because with reduced incomes they no longer consider it necessary to keep so large a balance. This "setting free" of money tends to restore incomes; and incomes not being contracted sufficiently, there is a deficit in the balance of trade and gold flows out. A recent German writer, Dr. Maier, treats gold flows somewhat similarly as a result of an excess of money balances at the equilibrium level of incomes. (See his book, *Goldwanderungen*.)

Whether one regards gold flows as the result of "setting free" balances rather than as the direct consequence of the falling off of exports, also whether or not one agrees with Mr. Harrod that the first process of income contraction is *instantaneous*, both seem to depend on a matter of convention. If one adopts the convention that incomes are earned and spent in the same period, these views appear to be correct; if one adopts the convention that incomes are earned in one period (Mr. Robertson's "day") and spent in the next, they are not. But there is no need to pursue this point. In relation to our practical issues, this analysis seems at first sight to make very little difference. So far as the final adjustment of incomes is brought about by gold flows (or for that matter by changes in exchange reserves), the process may be that outlined in the classical theory; gold flows affect reserves and the credit system has to adapt itself to changes in reserves by altering the terms of lending.

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[1] Mr. Harrod has developed his ideas further in *The Trade Cycle*. The present article was completed before I had opportunity to study his later work—hence the absence of any reference to it in the text.

But there is another possibility which gives this analysis great practical significance. This is that as each industry, from the export industry onwards, experiences a decline in the value of its output, there will be a reduction of bank loans and a cancellation of credit money. The balances which would otherwise be released to restore incomes would thus be extinguished and the " counteracting influence " noticed above removed. Gold movements and changes in reserves will occur, but they may on this view be accompaniments and not causes of the change in the volume of credit. The argument does not of course show that the changes in the volume of credit and in reserves will naturally tend to be proportional to one another. This may not matter if there is some flexibility in reserve ratios ; or the reserves may be adjusted to the credit volume, as suggested in the next section. In any case the extent to which the reserves control the volume of credit is on this view greatly reduced.

The significant point of difference between this and the classical view is that according to this the change in the volume of credit comes about, as it were, spontaneously, without being induced by a change in interest rates. Some might seek to minimise this point by arguing that in these circumstances the " natural rate " of interest falls, so that the maintenance of the same " money " rate is equivalent to a rise. Certainly, to use the old terminology, investment falls short of savings. But I think there is a difference between a change in the demand for loans due to a change in the aggregate demand for products, and one due to a change in the relationship between the market rate of interest and the marginal productivity of capital schedule. Take the case of an increase in the marginal productivity of capital, interest rates remaining the same (or a fall in interest rates, marginal productivity remaining the same) ; and assume, as is most appropriate in applying the natural rate of interest concept, that resources are fully employed. The outcome will be an increase of borrowing for the purpose of making production more capitalistic, and, assuming no change in voluntary saving, an immediate distortion, in the Austrian terminology, of the structure of production. On the other hand, if borrowing is increased as a result of an increase in aggregate effective demand from outside, it is the direction of the new demands which will determine the re-allocation of resources, and the bidding up of the prices of factors and materials by the borrowers will affect primarily a *horizontal* change in industry. It is true that this is not the whole story. Since factories and instruments of production are often not convertible from one purpose to another, the expansion of any industry, even if accompanied by the decline of another, is likely to call for new investment, to raise, in short, the schedule of marginal productivity of capital. Hence if interest rates remain the same, there will be some distortion of the structure of production, not so much because the capital developments in connexion with the expanding industry are inappropriate as because there will be insufficient curtailment of capital development in other directions. When this concession is made, however, I think there remains a substantial difference between the two types of case. And apparently it is not necessary to make a corresponding concession in the case of a decline in aggregate demand. It is true that in this case the production of capital goods is likely to fall off more than that of consumption goods during the period in which total output is declining, because all changes in total output tend to be reflected disproportionately in the production of capital goods. But this is analytically distinct from the kind of distortion envisaged in the Austrian theory. Once costs have been adjusted and full employment regained, the change in the direction of employment which is likely to be involved will have the same effect as in the opposite case (an increase in foreign demand) in raising the demand for capital. Hence it cannot be said that in this case the failure to *lower* interest rates introduces " structural distortion ".

In any case, leaving aside these abstruse questions of capital theory, adjustments in the volume of credit through changes in demand at the same rate of interest are likely to be more immediate than those induced by changes in the rate, and the complications resulting from discrepancies of interest rates between countries are avoided.

IV

Our theory must also take account of the possibility that gold movements, instead of being the determinants of the supply of money, may themselves be determined by monetary requirements. According to the strict quantity theory the demand for money is always adjusted to the supply through changes in its value, and monetary requirements must be the same as the actual supply. I do not need to discuss whether this is true for an isolated community; the suggestion is that in a regime of fixed exchange rates the monetary requirements of a particular country may be altered by changes in prices or trade activity independently of any prior change in the supply of money. If then the national supply of money is inelastic, owing, for example, to reserve requirements, an increase in monetary requirements will tend to raise interest rates, by curtailing the offer of funds for investment or increasing the demand for loans if the need is for larger bank balances, by curtailing bank reserves if the need is for cash in the narrower sense. Higher interest rates may promote a gold inflow through a movement of short term capital. (The same relationship between price levels, interest rates and monetary requirements can be seen, I think, in seasonal movements. Seasonal variations in monetary requirements appear to affect interest rates more than price levels.)

Evidence of this kind of relationship between gold movements and the monetary system is provided by the work of Professor A. Piatt Andrew and Professor Beach on the cyclical gold movements of the United States and the United Kingdom respectively. In both cases it has been found that gold flowed in during periods of active trade and rising prices, and flowed out in the opposite phase of the cycle. These facts are explicable on the orthodox theory only on the assumption that the countries concerned lagged behind the rest of the world in cyclical movements. It is highly improbable that this was the case with both the United States and the United Kingdom. The more reasonable interpretation is that these gold movements depended in the first instance on the relation of interest rates. Further evidence of concomitant movements of gold into and out of internal circulation confirms the view that it was the monetary requirements determined by a given price level which provided the underlying cause of the international gold movements. Presumably the countries which acted as the counter-parties to Britain and America in these movements were in a position to allow a considerable fluctuation in their gold reserves.

In the foregoing argument it is not intended to deny that the rate of interest exercised a controlling influence over the movement of prices and the general course of the trade cycle. High interest rates were doubtless of crucial importance in bringing booms to an end. What is contended is that in these particular cases the raising of interest rates did not have the effect of producing a *relative* reduction of prices in certain countries. High rates in London led rather to a world fall in prices, partly because of the sympathetic movement of rates elsewhere, partly because of the effect on British entrepôt trade and British long-term foreign investment.

The restriction of the argument to certain cases is also important. Professor Beach's study of British gold movements relates to the period from 1880 to 1914. It is possible that he would have obtained different results if he had taken an earlier period. The data have not, I think, been systematically studied, but one has the impression that in several of the earlier nineteenth century booms the expansion of credit in England outpaced any similar movement in other countries, with the result that there occurred an adverse balance of payments and à foreign drain. (I gather from his review in the *Statistical Journal* that Mr. Hawtrey considers that this may sometimes have happened in the course of the period studied by Professor Beach.) This is all in accordance with the classical line of thought, but it is also reconcilable with our alternative hypothesis. According to the argument of the preceding section, a change in the direction of demand which tends to turn the balance of trade against a country exercises at the same time a corrective deflationary influence. In the case of an inappropriate increase in home demand due to internal credit expansion, the effect on the balance of trade—the "leakage" of demand abroad—should curtail the tendency for demand to expand cumulatively. But if the original impulse to credit expansion continues to operate, the trade balance may remain unfavourable ; and gold may continue to flow out until the loss of reserves compels action to be taken. If the markets of the country are very closely linked with foreign markets, the loss of reserves by the ordinary banks should lead to an almost immediate correction of the expansive impulse, except in so far as a sympathetic movement is generated in the other countries ; for example there would appear to be little danger of a persistent tendency for credit to expand faster in Canada than in the United States. On the other hand, if the independent home market is relatively large, the original expansion may go on for some time and give rise to secondary expansions, before the full effect on the balance of payments is felt. In this case then there is need for anticipatory action and central bank intervention may be called for.

Our argument does not dispose, therefore, of the need for a national regulation of credit in order to prevent or correct an undue expansion in comparison with other countries. This regulation involves, directly or indirectly, the adjustment of interest rates; but in this case it can be said, I think, that the money rates are merely being kept in line with the natural rate,[1] and any movement of capital which results can be ascribed to an increase in the demand for capital.

V

There remains to be considered the process of adjustment in connexion with capital movements, assuming that the importing country requires additional real capital and not merely additional supplies of means of payment, as in the case considered at the beginning of the previous section.[2]

A transfer of capital involves in itself a redistribution of spending power, and this is likely to be accompanied by a change in the direction of demand. This is the basis for the view that the "transfer" process involves a change in the terms of trade. So far as this happens, there will have to be a change in the relationship between the price and income structures of the countries concerned; the factors of production will have become more valuable in some countries, less valuable in others. But this change will be of the same kind as that required by any other change in the direction of demand, and all that has been said in Section III above about the process of adjustment applies equally here.

The redistribution of spending power itself, however, apart from any change in the direction of demand and the terms of trade, may require a redistribution of money or means of payment. This is a point with respect to which there are certain differences of opinion. Some writers (Iversen, White, for example) hold that there must first be a redistribution of money, although it may be only a temporary one, in order that the redistribution of spending power shall take place at all. Others (including the present writer) think that the making of a foreign loan or investment itself shifts spending power *pro tanto*, but that this shift requires to be *accompanied* by a monetary redistribution if the process of transfer is to be smooth. (See my article in ECONOMICA of February, 1936.) Nurkse would apparently reject both these views and see no necessity for monetary changes apart from changes in the terms of trade.

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[1] I admit that in the case in which there is general unemployment of productive factors, this does not mean any more than the rate which equalises saving and investment at the existing level of incomes. But at least the maintenance of this rate is different from fixing a rate at which there is (or would be) an excess of saving with the existing level of incomes.

[2] The transition which has already been made to the present tense will warn the reader of the hypothetical character of the argument.

Assuming that the primary shift in spending power does call for a monetary transfer, the question arises as to how this may be brought about. Most obviously it may be effected by a movement of gold ; and if gold reserves are ample in the lending country, the process may be a simple and natural one. The lending community makes its surrender of wealth to the borrowers partly in the form of money, transferring this by means of a movement of gold. But if the gold movement necessitates a multiple contraction of credit in order to preserve the minimum reserve ratio in the lending country (accompanied perhaps by a multiple expansion of credit in the borrowing country), several complications arise. The change in money supplies will be excessive : interest rates will be distorted to bring about the credit changes : and the changes in interest rates will impede the international movement of capital. In some cases, of course, interest rates may be raised in the lending country with the deliberate object of checking the movement of capital and gold abroad. It is when interest rates are raised in these ways and not as a simple consequence of the transference of real capital, that it may be said that the demand for capital abroad has the effect of raising rates above the natural level.

There is, however, another way in which these monetary transfers may be brought about : that is, by creating additional money in the borrowing country on the basis of money held in the lending country. There is evidence, I think, that this was commonly done by the banks in many of the countries in which Britain invested—the Dominions and British colonies, the South American countries and Japan.

The lending country is in this way spared the loss of gold, yet the borrowing country gets increased supplies of money. If the banks in the borrowing country treat their foreign (or overseas) balances as being exactly equivalent to gold reserves, and proceed to make them the basis for a multiple expansion of their notes or deposits, the complications mentioned above will not, it is true, be entirely avoided. But they may be content to allow their foreign balances to increase relatively to their deposits; and I believe that the evidence again shows that the banks in the countries mentioned did commonly act in this way, restrained presumably by some instinctive recognition that any greater expansion of credit would soon have to be reversed.

The character of the process is also affected by whether the banks of the borrowing country continue to hold their external funds in liquid form or convert them into interest earning assets held in the lending centre. The ideal arrangement would appear to be for these banks to create local deposits of an amount exactly equal to their increased external balances and to hold these balances completely idle. This would give the closest possible approximation to the interregional shifts of circulation which take place within any one country. But the possibility that the external balances, instead of being "sterilised", will be invested, must be reckoned with. What this means is that spending power will not be directly reduced in the lending country by the full amount of the foreign (overseas) loans; that part of the surrender of wealth which would otherwise have taken the form of a surrender of money will not take place at all. If we can assume that all other countries (not merely the borrowing country) are prepared to base their money on that of the lending country, this failure to contract spending power in the latter can be compensated by a greater expansion of credit in the borrowing country, leading to a certain expansion in "neutral" countries also. That is to say, relative values will be the same as in the "ideal" process but money prices will be on a higher level. In this inflationary form the process of adaptation will probably be easier at the time of an increase of foreign lending, but correspondingly greater difficulties will be experienced in the debtor countries when the flow of one-sided payments has to be reversed, that is, when debt repayments and interest charges come to exceed new loans. So far as the condition mentioned above is not fulfilled and there are neutral or third party countries (possibly also some borrowers) which are not willing to base their money on that of the lending country, it seems inevitable that the latter should lose gold.

VI

It must be emphasised that the suggestions made in the preceding discussion—particularly the suggestion of a more direct adaptation of the volume of credit to what is required by international price relations than is contemplated in the classical theory—are no more than tentative. They provide a hypothetical view of the working of the gold standard which is more in accordance with my own impressions with regard to the historical facts than the accepted view; but it is admitted that my view requires a more exhaustive testing, with regard to both the cases in which the gold standard has worked successfully and those in which it has broken down, before it can claim acceptance.

In the meantime it may be useful to indicate certain consequences which would follow if my view were correct.

(1) Since gold movements (or more generally, changes in reserves) and discount rate adjustments are displaced from their central position in the process of international price adjustment, the question of "observing the rules of the game", as this is ordinarily understood, loses much of its importance. Indeed in some cases it will appear desirable that gold movements (changes in reserves) should not be accompanied by a multiple change in the volume of credit money;[1] and if the ordinary banks maintain a constant ratio between their liabilities and their balances at the central bank, this may require some offsetting of gold movements by the latter. With respect to discount rate variations, it is possible that the abnormal size of international short-term capital movements in recent years has been due in part to the excessive use of this method of regulating the international position.

(2) Whilst central bank policy is still important in certain respects, a new importance is given to the policy of the ordinary banks. If it is essential that the volume of credit should speedily adapt itself to changes in trade conditions,

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[1] This apart from the obvious case of temporary movements of gold.

a new justification is found for the otherwise rather discredited theory that notes and deposits should be covered by "self-liquidating" loans. (Perhaps the instincts of the Banking School were right on this point, too.) But the restriction of loans to the provision of working capital is not enough. For, as the reader has probably been objecting, it is quite possible that manufacturers faced with a declining demand will seek to increase their working capital in the form of stocks of unsold goods, at least for a time; and so far as the banks allow them to borrow for this purpose, the volume of credit will change in the wrong direction. (It is similarly possible that an improvement in trade will lead at first to a repayment of bank loans, but this is not very likely unless stocks have been abnormally large.) The behaviour of the banks in face of changing trade prospects is therefore of critical importance in my view, and what matters is not so much the rates of interest which they charge as their willingness to lend at all in certain cases. It would seem more probable that the banks would behave in the right way (from this point of view) if they are faced with gradual changes than if they are confronted suddenly with a situation seriously out of adjustment, *e.g.*, by stabilisation of the currency at too high a value.

(3) It has not been found necessary to refer in the preceding discussion to the question of income and cost rigidities. This only becomes relevant if we are considering not merely the rectification of the balance of payments but also the maintenance of full employment. One of the factors which is often supposed to be responsible for wage rigidity—the payment of unemployment allowances from public funds—may, however, have a bearing on the former issue. So far as the maintenance of the expenditure of the workers in depressed industries is not a mere transfer of expenditure, it must tend both to affect the volume of imports directly and to impair the cumulative process of income contraction.

Economics and Knowledge[1]

By F. A. von Hayek

I

The ambiguity of the title of this paper is not accidental. Its main subject is, of course, the rôle which assumptions and propositions about the knowledge possessed by the different members of society play in economic analysis. But this is by no means unconnected with the other question which might be discussed under the same title, the question to what extent formal economic analysis conveys any knowledge about what happens in the real world. Indeed my main contention will be that the tautologies, of which formal equilibrium analysis in economics essentially consists, can be turned into propositions which tell us anything about causation in the real world only in so far as we are able to fill those formal propositions with definite statements about how knowledge is acquired and communicated. In short I shall contend that the empirical element in economic theory—the only part which is concerned, not merely with implications but with causes and effects, and which leads therefore to conclusions which, at any rate in principle, are capable of verification[2]—consists of propositions about the acquisition of knowledge.

Perhaps I should begin by reminding you of the interesting fact that in quite a number of the more recent attempts made in different fields to push theoretical investigation beyond the limits of traditional equilibrium analysis, the answer has soon proved to turn on one question which, if not identical with mine, is at least part of it, namely the question of foresight. I think the field where, as one would expect, the discussion of the assumptions concerning foresight first attracted wider attention was the theory of risk.[3] The stimulus which was exercised in this connection by the work of Professor F. H. Knight may yet prove to have a profound influence far beyond its special field. Not much later the assumptions to be made concerning foresight proved to be of fundamental importance for the solution of the puzzles of the theory of imperfect competition, the questions of duopoly and oligopoly. And since then it has become more and more obvious that in the treatment of the more " dynamic " questions of money and industrial fluctuations the assumptions to be made about foresight and " anticipations " play an equally central rôle, and that in particular the concepts which were taken over into these fields from pure equilibrium analysis, like those of an equilibrium rate of interest, could be properly defined only in terms of assumptions concerning foresight. The situation seems here to be that before we can explain why people commit mistakes, we must first explain why they should ever be right.

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[1] Presidential Address to the London Economic Club, November 10th, 1936.

[2] Or rather falsification. Cf. K. Popper, *Logik der Forschung*, Vienna, 1935, *passim*.

[3] A more complete survey of the process by which the significance of anticipations was gradually introduced into economic analysis would probably have to begin with Professor Irving Fisher's *Appreciation and Interest* (1896).

In general it seems that we have come to a point where we all realise that the concept of equilibrium itself can be made definite and clear only in terms of assumptions concerning foresight, although we may not yet all agree what exactly these essential assumptions are. This question will occupy me later in this paper. At the moment I am only concerned to show that at the present juncture, whether we want to define the boundaries of economic statics or whether we want to go beyond it, we cannot escape the vexed problem of the exact position which assumptions about foresight are to have in our reasoning. Can this be merely an accident ?

As I have already suggested, the reason for this seems to me to be that we have to deal here only with a special aspect of a much wider question which we ought to have faced at a much earlier point. Questions essentially similar to those mentioned arise in fact as soon as we try to apply the system of tautologies—those series of propositions which are necessarily true because they are merely transformations of the assumptions from which we start, and which constitute the main content of equilibrium analysis[1]—to the situation of a society consisting of several independent persons. I have long felt that the concept of equilibrium itself and the methods which we employ in pure analysis, have a clear meaning only when confined to the analysis of the action of a single person, and that we are really passing into a different sphere and silently introducing a new element of altogether different character when we apply it to the explanation of the interactions of a number of different individuals.

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[1] I should like to make it clear from the outset that I use the term " equilibrium analysis " throughout this paper in the narrower sense in which it is equivalent to what Professor Hans Mayer has christened the " functional " (as distinguished from the " causal- genetic ") approach, and to what used to be loosely described as the " mathematical school ". It is round this approach that most of the theoretical discussions of the past ten or fifteen years have taken place. It is true that Professor Mayer has held out before us the prospect of another, " causal-genetic " approach, but it can hardly be denied that this is still largely a promise. It should,

I am certain there are many who regard with impatience and distrust the whole tendency, which is inherent in all modern equilibrium analysis, to turn economics into a branch of pure logic, a set of self-evident propositions which, like mathematics or geometry, are subject to no other test but internal consistency. But it seems that if only this process is carried far enough it carries its own remedy with it. In distilling from our reasoning about the facts of economic life those parts which are truly *a priori*, we not only isolate one element of our reasoning as a sort of Pure Logic of Choice in all its purity, but we also isolate, and emphasise the importance of, another element which has been too much neglected. My criticism of the recent tendencies to make economic theory more and more formal is not that they have gone too far, but that they have not yet been carried far enough to complete the isolation of this branch of logic and to restore to its rightful place the investigation of causal processes, using formal economic theory as a tool in the same way as mathematics.

II

But before I can prove my contention that the tautological propositions of pure equilibrium analysis as such are not directly applicable to the explanation of social relations, I must first show that the concept of equilibrium *has* a clear meaning if applied to the actions of a single individual, and what this meaning is. Against my contention it might be argued that it is precisely here that the concept of equilibrium is of no significance, because, if one

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however, be mentioned here that some of the most stimulating suggestions on problems closely related to those treated here have come from his circle. Cf., H. Mayer, "Der Erkenntniswert der funktionellen Preistheorien," *Die Wirtschaftstheorie der Gegenwart*, Vol. II, 1931; P. N. Rosenstein-Rodan, "Das Zeitmoment in der mathematischen Theorie des wirtschaftlichen Gleichgewichts," *Zeitschrift für Nationalökonomie*, Vol. I, No. 1, and "The Rôle of Time in Economic Theory," ECONOMICA (N.S.), Vol. I (1), 1934.

wanted to apply it, all one could say would be that an isolated person was always in equilibrium. But this last statement, although a truism, shows nothing but the way in which the concept of equilibrium is typically misused. What is relevant is not whether a person as such is or is not in equilibrium, but which of his actions stand in equilibrium relationships to each other. All propositions of equilibrium analysis, such as the proposition that relative values will correspond to relative costs, or that a person will equalise the marginal returns of any one factor in its different uses, are propositions about the relations between actions. Actions of a person can be said to be in equilibrium in so far as they can be understood as part of one plan. Only if this is the case, only if all these actions have been decided upon at one and the same moment, and in consideration of the same set of circumstances, have our statements about their interconnections, which we deduce from our assumptions about the knowledge and the preferences of the person, any application. It is important to remember that the so-called "data", from which we set out in this sort of analysis, are (apart from his tastes) all facts given to the person in question, the things as they are known to (or believed by) him to exist, and not in any sense objective facts. It is only because of this that the propositions we deduce are necessarily *a priori* valid, and that we preserve the consistency of the argument.[1]

The two main conclusions from these considerations are, *firstly*, that since equilibrium relations exist between the successive actions of a person only in so far as they are part of the execution of the same plan, any change in the relevant knowledge of the person, that is, any change which leads him to alter his plan, disrupts the equilibrium relation between his actions taken before and those taken after the change in his knowledge. In other words, the equilibrium relationship comprises only his actions during the period during which his anticipations prove correct. *Secondly*, that since equilibrium is a relationship between actions, and since the actions of one person must necessarily take place successively in time, it is obvious that the passage of time is essential to give the concept of equilibrium any meaning. This deserves mention since many economists appear to have been unable to find a place for time in equilibrium analysis and consequently have suggested that equilibrium must be conceived as timeless. This seems to me to be a meaningless statement.

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[1] Cf., on this point particularly L. Mises, *Grundprobleme der Nationalökonomie*, Jena, 1933, pp. 22 *et seq.*, 160 *et seq.*

III

Now, in spite of what I have said before about the doubtful meaning of equilibrium analysis in this sense if applied to the conditions of a competitive society, I do not of course want to deny that the concept was originally introduced precisely to describe the idea of some sort of balance between the actions of different individuals. All I have argued so far is that the sense in which we use the concept of equilibrium to describe the interdependence of the different actions of one person does not immediately admit of application to the relations between actions of different people. The question really is what use we make of it when we speak of equilibrium with reference to a competitive system.

The first answer which would seem to follow from our approach is that equilibrium in this connection exists if the actions of all members of the society over a period are all executions of their respective individual plans on which each decided at the beginning of the period. But when we inquire further what exactly this implies, it appears that this answer raises more difficulties than it solves. There is no special difficulty about the concept of an isolated person (or a group of persons directed by one of them) acting over a period according to a preconceived plan. In this case, the execution of the plan need not satisfy any special criteria in order to be conceivable. It may of course be based on wrong assumptions concerning the external facts and on this account may have to be changed. But there will always be a conceivable set of external events which would make it possible for the plan to be executed as originally conceived.

The situation is, however, different with the plans determined upon simultaneously but independently by a number of persons. In the first instance, in order that all these plans can be carried out, it is necessary for them to be based on the expectation of the same set of external events, since, if different people were to base their plans on conflicting expectations, no set of external events could make the execution of all these plans possible. And, secondly, in a society based on exchange their plans will to a considerable extent refer to actions which require corresponding actions on the part of other individuals. This means that the plans of different individuals must in a special sense be compatible if it is to be even conceivable that they will be able to carry all of them out.[1] Or, to put the same thing in different words, since some of the " data " on which any one person will base his plans will be the expectation that other people will act in a particular way, it is essential for the compatibility of the different plans that the plans of the one contain exactly those actions which form the data for the plans of the other.

In the traditional treatment of equilibrium analysis part of this difficulty is apparently avoided by the assumption that the data, in the form of demand schedules representing individual tastes and technical facts, will be equally given to all individuals and that their acting on the same premises will somehow lead to their plans becoming adapted to each other. That this does not really overcome the difficulty created by the fact that one person's decisions are the other person's data, and that it involves to some degree circular reasoning, has often been pointed out. What, however, seems so far to have escaped notice is that this whole procedure involves a confusion of a much more general character, of which the point just mentioned is just a special instance, and which is due to an equivocation of the term " datum ". The data which now are supposed to be objective facts and the same for all people are evidently no longer the same thing as the data which formed the starting point for the tautological transformations of the Pure Logic of Choice. There " data " meant all facts, and only the facts, which were present in the mind of the acting person, and only this subjective interpretation of the term datum made those propositions necessary truths. " Datum " meant given, known, to the person under consideration. But in the transition from the analysis of the action of an individual to the analysis of the situation in a society the concept has undergone an insidious change of meaning.

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[1] It has long been a subject of wonder to me why there should, to my knowledge, have been no systematic attempts in sociology to analyse social relations in terms of correspondence and non-correspondence, or compatibility and non-compatibility, of individual aims and desires. It seems that the mathematical technique of *analysis situs* (topology) and particularly such concepts developed by it as that of *homeomorphism* might prove very useful in this connection, although it may appear doubtful whether even this technique, at any rate in the present state of its development, is adequate to the complexity of the structures with which we have to deal. A first attempt made recently in this direction by an eminent mathematician (Karl Menger, *Moral, Wille und Weltgestaltung*, Vienna, 1934) has so far not yet led to very illuminating results. But we may look forward with interest to the treatise on exact sociological theory which Professor Menger has promised for the near future. (Cf.," Einige neuere Fortschritte in der exakten Behandlung sozialwissenschaftlicher Probleme," in *Neuere Fortschritte in den exakten Wissenschaften*, Vienna, 1936, p. 132.)

IV

The confusion about the concept of a datum is at the bottom of so many of our difficulties in this field that it is necessary to consider it in somewhat more detail. Datum means of course something given, but the question which is left open, and which in the social sciences is capable of two different answers, is to whom the facts are supposed to be given. Economists appear subconsciously always to have been somewhat uneasy about this point, and to have reassured themselves against the feeling that they did not quite know to whom the facts were given by underlining the fact that they *were* given — even by using such pleonastic expressions as "given data". But this does not solve the question whether the facts referred to are supposed to be given to the observing economist, or to the persons whose actions he wants to explain, and if to the latter, whether it is assumed that the same facts are known to all the different persons in the system, or whether the "data" for the different persons may be different.

There seems to be no possible doubt that these two concepts of "data", on the one hand in the sense of the objective real facts, as the observing economist is supposed to know them, and on the other in the subjective sense, as things known to the persons whose behaviour we try to explain, are really fundamentally different and ought to be kept carefully apart. And, as we shall see, the question why the data in the subjective sense of the term should ever come to correspond to the objective data is one of the main problems we have to answer.

The usefulness of the distinction becomes immediately apparent when we apply it to the question of what we can mean by the concept of a society being at any one moment in a state of equilibrium. There are evidently two senses in which it can be said that the subjective data, given to the different persons, and the individual plans, which necessarily follow from them, are in agreement. We may merely mean that these plans are mutually compatible and that there is consequently a conceivable set of external events which will allow all people to carry out their plans and not cause any disappointments. If this mutual compatibility of intentions were not given, and if in consequence no set of external events could satisfy all expectations, we could clearly say that this is not a state of equilibrium. We have a situation where a revision of the plans on the part of at least some people is inevitable, or, to use a phrase which in the past has had a rather vague meaning, but which seems to fit this case perfectly, where endogenous disturbances are inevitable.

There is, however, still the other question of whether the individual subjective sets of data correspond to the objective data, and whether in consequence the expectations on which plans were based are borne out by the facts. If correspondence between data in this sense were required for equilibrium it would never be possible to decide otherwise than *ex post*, at the end of the period for which people have planned, whether at the beginning the society has been in equilibrium. It seems to be more in conformity with established usage to say in such a case that the equilibrium, as defined in the first sense, may be disturbed by an unforeseen development of the (objective) data, and to describe this as an exogenous disturbance. In fact it seems hardly possible to attach any definite meaning to the much used concept of a change in the (objective) data unless we distinguish between external developments in conformity with, and those different from, general expectations, and define as a " change " any divergence of the actual from the expected development, irrespective of whether it means a " change " in some absolute sense. Surely if the alternations of the seasons suddenly ceased and the weather remained constant from a certain day onward, this would represent a change of data in our sense, that is a change relative to expectations, although in an absolute sense it would not represent a change but rather an absence of change. But all this means that we can speak of a change in data only if equilibrium in the first sense exists, that is, if expectations coincide. If they conflicted, any development of the external facts might bear out somebody's expectations and disappoint those of others, and there would be no possibility of deciding what was a change in the objective data.[1]

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[1] Cf. " The Maintenance of Capital," ECONOMICA (N.S.), Vol. II, 1935, p. 265.

V

For a society then we *can* speak of a *state* of equilibrium at a point of time—but it means only that compatibility exists between the different plans which the individuals composing it have made for action in time. And equilibrium will continue, once it exists, so long as the external data correspond to the common expectations of all the members of the society. The continuance of a state of equilibrium in this sense is then not dependent on the objective data being constant in an absolute sense, and is not necessarily confined to a stationary process. Equilibrium analysis becomes in principle applicable to a progressive society and to those inter-temporal price relationships which have given us so much trouble in recent times.[1]

These considerations seem to throw considerable light on the relationship between equilibrium and foresight, which has been somewhat hotly debated in recent times.[2] It appears that the concept of equilibrium merely means that the foresight of the different members of the society is in a special sense correct. It must be correct in the sense that every person's plan is based on the expectation of just those actions of other people which those other people intend to perform, and that all these plans are based on the expectation of the same set of external facts, so that under certain conditions nobody will have any reason to change his plans. Correct foresight is then not, as it has sometimes been understood, a precondition which must exist in order that equilibrium may be arrived at. It is rather the defining characteristic of a state of equilibrium. Nor need foresight for this purpose be perfect in the sense that it need extend into the indefinite future, or that everybody must foresee everything correctly. We should rather say that equilibrium will last so long as the anticipations prove correct, and that they need to be correct only on those points which are relevant for the decisions of the individuals. But on this question of what is relevant foresight or knowledge, more later.

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[1] This separation of the concept of equilibrium from that of a stationary state seems to me to be no more than the necessary outcome of a process which has been going on for a fairly long time. That this association of the two concepts is not essential but only due to historical reasons is to-day probably generally felt. If complete separation has not yet been effected, it is apparently only because no alternative definition of a state of equilibrium had yet been suggested which has made it possible to state in a general form those propositions of equilibrium analysis which are essentially independent of the concept of a stationary state. Yet it is evident that most of the propositions of equilibrium analysis are not supposed to be applicable only in that stationary state which will probably never be reached. The process of separation seems to have begun with Marshall and his distinction between long and short run equilibria. (Cf., statements like this: "For the nature of equilibrium itself, and that of the causes by which it is determined, depend on the length of the period over which the market is taken to extend." *Principles*, Vol. I, 6, 7th ed., p. 330.) The idea of a state of equilibrium which was not a stationary state was already inherent in my "Das intertemporale Gleichgewichtssystem der Preise und die Bewegungen des Geldwertes" (*Weltwirtschaftliches Archiv*, Vol. XXVIII, June, 1928) and is, of course, essential if we want to use the equilibrium apparatus for the explanation of any of the phenomena connected with "investment". On the whole matter much historical information will be found in E. Schams, Komparative Statistik, *Zeitschrift für Nationalökonomie* II/1, 1930.

[2] Cf. particularly O. Morgenstern, "Vollkommene Voraussicht und Wirtschaftliches Gleichgewicht," *Zeitschrift für Nationalökonomie*, Vol. VI, p. 3.

Before I proceed further I should probably stop for a moment to illustrate by a concrete example what I have just said about the meaning of a state of equilibrium and how it can be disturbed. Consider the preparations which will be going on at any moment for the production of houses. Brickmakers, plumbers and others will all be producing materials which in each case will correspond to a certain quantity of houses for which just this quantity of the particular material will be required. Similarly we may conceive of prospective buyers as accumulating savings which will enable them at certain dates to buy definite quantities of houses. If all these activities represent preparations for the production (and acquisition) of the same amount of houses we can say that there is equilibrium between them in the sense that all the people engaged in them may find that they can carry out their plans.[1] This need not be so, because other circumstances which are not part of their plan of action may turn out to be different from what they expected. Part of the materials may be destroyed by an accident, weather conditions may make building impossible, or an invention may alter the proportions in which the different factors are wanted. This is what we call a change in the (objective) data, which disturbs the equilibrium which has existed. But if the different plans were from the beginning incompatible, it is inevitable that somebody's plans will be upset and have to be altered, and that in consequence the whole complex of actions over the period will not show those characteristics which apply if all the actions of each individual can be understood as part of a single individual plan he has made at the beginning.[1]

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[1] Another example of more general importance would, of course, be the correspondence between " investment " and " saving " in the sense of the proportion (in terms of relative cost) in which entrepreneurs provide producers' goods and consumers' goods for a particular date, and the proportion in which consumers in general will at this date distribute their resources between producers' goods and consumers' goods. (Cf. my " Preiserwartungen, monetäre Störungen und Fehlinvestitionen," *Ekonomisk Tidskrift*, Vol. 34, 1935 (French translation: " Prévisions de Prix, Perturbations Monétaires et Faux Investissements," *Revue des Sciences Economiques*, October, 1935) and " The Maintenance of Capital," ECONOMICA (N.S.), Vol. II, 1935, pp. 268–273.) It may be of interest in this connection to mention that in the course of investigations of the same field, which led the present author to these speculations, the theory of crises, the great French sociologist G. Tarde stressed the " contradiction de croyances " or " contradiction de jugements " or " contradictions des espérances " as the main cause of these phenomena (*Psychologie Economique*, Paris, 1902, Vol. II, pp. 128–9; Cf. also N. Pinkus, *Das Problem des Normalen in der Nationalökonomie*, Leipzig, 1906, pp. 252 and 275).

VI

When in all this I emphasise the distinction between mere inter-compatibility of the individual plans[2] and the correspondence between them and the actual external facts or objective data, I do not of course mean to suggest that the subjective inter-agreement is not in some way brought about by the external facts. There would of course be no reason why the subjective data of different people should ever correspond unless they were due to the experience of the same objective facts. But the point is that pure equilibrium analysis is not concerned with the way in which this correspondence is brought about. In the description of an existing state of equilibrium which it provides, it is simply assumed that the subjective data coincide with the objective facts. The equilibrium relationships cannot be deduced merely from the objective facts, since the analysis of what people will do can only start from what is known to them. Nor can equilibrium analysis start merely from a given set of subjective data, since the subjective data of different people would be either compatible or incompatible, that is, they would already determine whether equilibrium did or did not exist.

We shall not get much further here unless we ask for the reasons for our concern with the admittedly fictitious state of equilibrium. Whatever may occasionally have been said by over-pure economists, there seems to be no possible doubt that the only justification for this is the supposed existence of a tendency towards equilibrium. It is only with this assertion that economics ceases to be an exercise in pure logic and becomes an empirical science; and it is to economics as an empirical science that we must now turn.

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[1] It is an interesting question, but one which I cannot discuss here, whether in order that we can speak of equilibrium, every single individual must be right, or whether it would not be sufficient if, in consequence of a compensation of errors in different directions, quantities of the different commodities coming on the market were the same as if every individual had been right. It seems to me as if equilibrium in the strict sense would require the first condition to be satisfied, but I can conceive that a wider concept, requiring only the second condition, might occasionally be useful. A fuller discussion of this problem would have to consider the whole question of the significance which some economists (including Pareto) attach to the law of great numbers in this connection. On the general point see P. N. Rosenstein-Rodan, "The Coordination of the General Theories of Money and Price," ECONOMICA, August, 1936.

[2] Or, since in view of the tautological character of the Pure Logic of Choice, "individual plans" and "subjective data" can be used interchangeably, between the agreement between the subjective data of the different individuals.

In the light of our analysis of the meaning of a state of equilibrium it should be easy to say what is the real content of the assertion that a tendency towards equilibrium exists. It can hardly mean anything but that under certain conditions the knowledge and intentions of the different members of society are supposed to come more and more into agreement, or, to put the same thing in less general and less exact but more concrete terms, that the expectations of the people and particularly of the entrepreneurs will become more and more correct. In this form the assertion of the existence of a tendency towards equilibrium is clearly an empirical proposition, that is, an assertion about what happens in the real world which ought, at least in principle, to be capable of verification. And it gives our somewhat abstract statement a rather plausible common-sense meaning. The only trouble is that we are still pretty much in the dark about (*a*) the *conditions* under which this tendency is supposed to exist, and (*b*) the nature of the *process* by which individual knowledge is changed.

VII

In the usual presentations of equilibrium analysis it is generally made to appear as if these questions of how the equilibrium comes about were solved. But if we look closer it soon becomes evident that these apparent demonstrations amount to no more than the apparent proof of what is already assumed.[1] The device generally adopted for this purpose is the assumption of a perfect market where every event becomes known instantaneously to every member. It is necessary to remember here that the perfect market which is required to satisfy the assumptions of equilibrium analysis must not be confined to the markets of all the individual commodities; the whole economic system must be assumed to be one perfect market in which everybody knows everything. The assumption of a perfect market then means nothing less than that all the members of the community, even if they are not supposed to be strictly omniscient, are at least supposed to know automatically all that is relevant for their decisions. It seems that that skeleton in our cupboard, the "economic man", whom we have exorcised with prayer and fasting, has returned through the back door in the form of a quasi-omniscient individual.

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[1] This seems to be implicitly admitted, although hardly consciously recognised, when in recent times it is frequently stressed that equilibrium analysis only describes the conditions of equilibrium without attempting to derive the position of equilibrium from the data. Equilibrium analysis in this sense would, of course, be pure logic and contain no assertions about the real world.

The statement that, if people know everything, they are in equilibrium is true simply because that is how we define equilibrium. The assumption of a perfect market in this sense is just another way of saying that equilibrium exists, but does not get us any nearer an explanation of when and how such a state will come about. It is clear that if we want to make the assertion that under certain conditions people will approach that state we must explain by what process they will acquire the necessary knowledge. Of course any assumption about the actual acquisition of knowledge in the course of this process will also be of a hypothetical character. But this does not mean that all such assumptions are equally justified. We have to deal here with assumptions about causation, so that what we assume must not only be regarded as possible (which is certainly not the case if we just regard people as omniscient) but must also be regarded as likely to be true, and it must be possible, at least in principle, to demonstrate that it is true in particular cases.

The essential point here is that it is these apparently subsidiary hypotheses or assumptions that people do learn from experience, and about how they acquire knowledge, which constitute the empirical content of our propositions about what happens in the real world. They usually appear disguised and incomplete as a description of the type of market to which our proposition refers; but this is only one, though perhaps the most important, aspect of the more general problem of how knowledge is acquired and communicated. The important thing of which economists frequently do not seem to be aware is that the nature of these hypotheses is in many respects rather different from the more general assumptions from which the Pure Logic of Choice starts. The main differences seem to me to be two:

Firstly, the assumptions from which the Pure Logic of Choice starts are facts which we know to be common to all human thought. They may be regarded as axioms which define or delimit the field within which we are able to understand or mentally to reconstruct the processes of thought of other people. They are therefore universally applicable to the field in which we are interested—although of course where *in concreto* the limits of this field are is an empirical question. They refer to a type of human action (what we commonly call rational, or even merely conscious, as distinguished from instinctive action) rather than to the particular conditions under which this action is undertaken. But the assumptions or hypotheses, which we have to introduce when we want to explain the social processes, concern the relation of the thought of an individual to the outside world, the question to what extent and how his knowledge corresponds to the external facts. And the hypotheses must necessarily run in terms of assertions about causal connections, about how experience creates knowledge.

Secondly, while in the field of the Pure Logic of Choice our analysis can be made exhaustive, that is, while we can here develop a formal apparatus which covers all conceivable situations, the supplementary hypotheses must of necessity be selective, that is, we must select from the infinite variety of possible situations such ideal types as for some reason we regard as specially relevant to conditions in the real world.[1] Of course we could also develop a separate science, the subject matter of which was *per definitionem* confined to a " perfect market " or some similarly defined object, just as the Logic of Choice applies only to persons who have to allot limited means among a variety of ends. And for the field so defined our propositions would again become *a priori* true. But for such a procedure we should lack the justification which consists in the assumption that the situation in the real world is similar to what we assume it to be.

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[1] The distinction drawn here may help to solve the old difference between economists and sociologists about the rôle which " ideal types " play in the reasoning of economic theory. The sociologists used to emphasise that the usual procedure of economic theory involved the assumption of particular ideal types, while the economic theorist pointed out that his reasoning was of such generality that he need not make use of any " ideal types ". The truth seems to be that within the field of the Pure Logic of Choice, in which the economist was largely interested, he was right in his assertion, but that as soon as he wanted to use it for the explanation of a social process he had to use " ideal types " of one sort or another.

VIII

I must now turn to the question of what the concrete hypotheses are concerning the conditions under which people are supposed to acquire the relevant knowledge and the process by which they are supposed to acquire it. If it were at all clear what the hypotheses usually employed in this respect were, we should have to scrutinise them in two respects: we should have to investigate whether they were necessary and sufficient to explain a movement towards equilibrium, and we should have to show to what extent they were borne out by reality. But I am afraid I am now getting to a stage where it becomes exceedingly difficult to say what exactly are the assumptions on the basis of which we assert that there will be a tendency towards equilibrium, and to claim that our analysis has an application to the real world. I cannot pretend that I have as yet got much further on this point. Consequently all I can do is to ask a number of questions to which we shall have to find an answer if we want to be clear about the significance of our argument.

The only condition, about the necessity of which for the establishment of an equilibrium economists seem to be fairly agreed, is the " constancy of the data ". But after what we have seen about the vagueness of the concept of " datum " we shall suspect, and rightly, that this does not get us much farther. Even if we assume—as we probably must—that here the term is used in its objective sense (which includes, it will be remembered, the preferences of the different individuals) it is by no means clear that this is either required or sufficient in order that people shall actually acquire the necessary knowledge, or that it was meant as a statement of the conditions under which they will do so. It is rather significant that at any rate some authors[1] feel it necessary to add " perfect knowledge " as an additional and separate condition. And indeed we shall see that constancy of the objective data is neither a necessary nor a sufficient condition. That it cannot be a necessary condition follows from the facts, firstly, that nobody would want to interpret it in the absolute sense that nothing must ever happen in the world, and, secondly, that, as we have seen, as soon as we want to include changes which occur periodically or perhaps even changes which proceed at a constant rate, the only way in which we can define constancy is with reference to expectations. All that this condition amounts to then is that there must be some discernible regularity in the world which makes it possible to predict events correctly. But while this is clearly not sufficient to prove that people will learn to foresee events correctly, the same is true to a hardly less degree even about constancy of data in an absolute sense. For any one individual, constancy of the data does in no way mean constancy of all the facts independent of himself, since, of course, only the tastes and not the actions of the other people can in this sense be assumed to be constant. And as all those other people will change their decisions as they gain experience about the external facts and other people's action, there is no reason why these processes of successive changes should ever come to an end. These difficulties are well known[1] and I only mention them here to remind you how little we actually know about the conditions under which an equilibrium will ever be reached. But I do not propose to follow this line of approach further, though not because this question of the empirical probability that people will learn (that is, that their subjective data will come to correspond with each other and with the objective facts) is lacking in unsolved and highly interesting problems. The reason is rather that there seems to me to be another and more fruitful way of approach to the central problem.

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[1] *Vide* N. Kaldor, " A Classificatory Note on the Determinateness of Equilibrium," *Review of Economic Studies*, Vol. I, No. 2, 1934, p. 123.

IX

The questions I have just discussed concerning the conditions under which people are likely to acquire the necessary knowledge, and the process by which they will acquire it, has at least received some attention in past discussions. But there is a further question which seems to me to be at least equally important, but which appears to have received no attention at all, and that is how much knowledge and what sort of knowledge the different individuals must possess in order that we may be able to speak of equilibrium. It is clear that if the concept is to have any empirical significance it cannot presuppose that everybody knows everything. I have already had to use the undefined term " relevant knowledge ", that is, the knowledge which is relevant to a particular person. But what is this relevant knowledge? It can hardly mean simply the knowledge which actually influenced his actions, because his decisions might have been different not only if, for instance, the knowledge he possessed had been correct instead of incorrect, but also if he had possessed knowledge about altogether different fields.

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[1] On all this cf. N. Kaldor, loc. cit., *passim*.

Clearly there is here a problem of the *Division of Knowledge* which is quite analogous to, and at least as important as, the problem of the division of labour. But while the latter has been one of the main subjects of investigation ever since the beginning of our science, the former has been as completely neglected, although it seems to me to be the really central problem of economics as a social science.[1] The problem which we pretend to solve is how the spontaneous interaction of a number of people, each possessing only bits of knowledge, brings about a state of affairs in which prices correspond to costs, *etc*., and which could be brought about by deliberate direction only by somebody who possessed the combined knowledge of all those individuals. And experience shows us that something of this sort does happen, since the empirical observation that prices do tend to correspond to costs was the beginning of our science. But in our analysis, instead of showing what bits of information the different persons must possess in order to bring about that result, we fall in effect back on the assumption that everybody knows everything and so evade any real solution of the problem.

Before, however, we can proceed further, to consider this division of knowledge among different persons, it is necessary to become more specific about the sort of knowledge which is relevant in this connection. It has become customary among economists to stress only the need of knowledge of prices, apparently because—as a consequence of the confusions between objective and subjective data—the complete knowledge of the objective facts was taken for granted. In recent times even the knowledge of current prices has been taken so much for granted that the only connection in which the question of knowledge has been regarded as problematic has been the anticipation of future prices. But, as I have already indicated at the beginning, price expectations and even the knowledge of current prices are only a very small section of the problem of knowledge as I see it. The wider aspect of the problem of knowledge with which I am concerned is the knowledge of the basic fact of how the different commodities can be obtained and used,[1] and under what conditions they are actually obtained and used, that is, the general question of why the subjective data to the different persons correspond to the objective facts. Our problem of knowledge here is just the existence of this correspondence which in much of current equilibrium analysis is simply assumed to exist, but which we have to explain if we want to show why the propositions, which are necessarily true about the attitude of a person towards things which he believes to have certain properties, should come to be true of the actions of society with regard to things which either do possess these properties, or which, for some reason we shall have to explain, are commonly believed by the members of society to possess these properties.[2]

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[1] I am not certain, but I hope, that the distinction between the Pure Logic of Choice and economics as a social science is essentially the same distinction as that which Professor A. Ammon has in mind when he stresses again and again that a " *Theorie des Wirtschaftens* " is not yet a " *Theorie der Volkswirtschaft* ".

But to revert to the special problem I have been discussing, the amount of knowledge different individuals must possess in order that equilibrium may prevail (or the "relevant" knowledge they must possess), we shall get nearer to an answer if we remember how it can become apparent either that equilibrium did not exist or that it is being disturbed. We have seen that the equilibrium connections will be severed if any person changes his plans, either because his tastes change (which does not concern us here) or because new facts become known to him. But there are evidently two different ways in which he may learn of new facts which make him change his plans, which for our purposes are of altogether different significance. He may learn of the new facts as it were by accident, that is in a way which is not a necessary consequence of his attempt to execute his original plan, or it may be inevitable that in the course of his attempt he will find that the facts are different from what he expected. It is obvious that, in order that he may proceed according to plan, his knowledge needs to be correct only on the points on which it will necessarily be confirmed or corrected in the course of the execution of the plan. But he may have no knowledge of things which, if he possessed it, would certainly affect his plan.

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[1] Knowledge in this sense is more than what is usually described as skill, and the division of knowledge of which we here speak more than is meant by the division of labour. To put it shortly, "skill" refers only to the knowledge of which a person makes use in his trade, while the further knowledge about which we must know something in order to be able to say anything about the processes in society, is the knowledge of alternative possibilities of action of which he makes no direct use. It may be added here that knowledge, in the sense in which the term is here used, is identical with foresight only in the sense in which all knowledge is capacity to predict.

[2] That all propositions of economic theory refer to things which are defined in terms of human attitudes towards them, that is, that for instance the "sugar" about which economic theory may occasionally speak, is not defined by its "objective" qualities, but by the fact that people believe that it will serve certain needs of theirs in a certain way, is the source of all sorts of difficulties and confusions, particularly in connection with the problem of "verification". It is, of course, also in this connection that the contrast between the *verstehende* social science and the behaviourist approach becomes so glaring. I am not certain that the behaviourists in the social sciences are quite aware of *how* much of the traditional approach they would have to abandon if they wanted to be consistent, or that they would want to adhere to it consistently if they were aware of this. It would, for instance, imply that propositions of the theory of money would have to refer exclusively to, say, "round discs of metal, bearing a certain stamp," or some similarly defined physical object or group of objects.

The conclusion then which we must draw is that the relevant knowledge which he must possess in order that equilibrium may prevail is the knowledge which he is bound to acquire in view of the position in which he originally is, and the plans which he then makes. It is certainly not all the knowledge which, if he acquired it by accident, would be useful to him, and lead to a change in his plan. And we may therefore very well have a position of equilibrium only because some people have no chance of learning about facts which, if they knew them, would induce them to alter their plans. Or, in other words, it is only relative to the knowledge which a person is bound to acquire in the course of the carrying out of his original plan and its successive alterations that an equilibrium is likely to be reached.

While such a position represents in one sense a position of equilibrium, it is however clear that it is not an equilibrium in the special sense in which equilibrium is regarded as a sort of optimum position. In order that the results of the combination of individual bits of knowledge should be comparable to the results of direction by an omniscient dictator, further conditions must apparently be introduced.[1] And while it seems quite clear that it is possible to define the amount of knowledge which individuals must possess in order that this result should be obtained, I know of no real attempt in this direction. One condition would probably be that each of the alternative uses of any sort of resources is known to the owner of some such resources actually used for another purpose and that in this way all the different uses of these resources are connected, either directly or indirectly.[1] But I mention this condition only as an instance of how it will in most cases be sufficient that in each field there is a certain margin of people who possess among them all the relevant knowledge. To elaborate this further would be an interesting and a very important task, but a task that would far exceed the limits of this paper.

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[1] These conditions are usually described as absence of " frictions ". In a recently published article (" Quantity of Capital and the Rate of Interest," *Journal of Political Economy*, Vol. XLIV/5, 1936, p. 638) Professor F. H. Knight rightly points out that " ' error ' is the usual meaning of friction in economic discussion ".

But although what I have said on this point has been largely in the form of a criticism, I do not want to appear unduly despondent about what we have already achieved in this field. Even if we have jumped over an essential link in our argument, I still believe that by what is implicit in its reasoning, economics has come nearer than any other social science to an answer to that central question of all social sciences, how the combination of fragments of knowledge existing in different minds can bring about results which, if they were to be brought about deliberately, would require a knowledge on the part of the directing mind which no single person can possess. To show that in this sense the spontaneous actions of individuals will under conditions which we can define bring about a distribution of resources which can be understood as if it were made according to a single plan, although nobody has planned it, seems to me indeed an answer to the problem which has sometimes been metaphorically described as that of the "social mind". But we must not be surprised that such claims on our part have usually been rejected by sociologists, since we have not based them on the right grounds.

There is only one more point in this connection which

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[1] This would be one, but probably not yet a sufficient, condition to ensure that, with a given state of demand, the marginal productivity of the different factors of production in their different uses should be equalised and that in this sense an equilibrium of production should be brought about. That it is not necessary, as one might think, that every possible alternative use of any kind of resources should be known to at least one among the owners of each group of such resources which are used for one particular purpose is due to the fact that the alternatives known to the owners of the resources in a particular use are reflected in the prices of these resources. In this way it may be a sufficient distribution of knowledge of the alternative uses, $m, n, o, \ldots y, z$, of a commodity, if A, who uses the quantity of these resources in his possession for m, knows of n, and B, who uses his for n, knows of m, while C who uses his for o, knows of n, *etc.*, *etc.*, until we get to L, who uses his for z, but only knows of y. I am not clear to what extent in addition to this a particular distribution of the knowledge of the different proportions is required in which different factors can be combined in the production of any one commodity. For complete equilibrium additional assumptions will be required about the knowledge which consumers possess about the serviceability of the commodities for the satisfaction of their wants.

I should like to mention. This is that if the tendency towards equilibrium, which we have reason to believe to exist on empirical grounds, is only towards an equilibrium relative to that knowledge which people will acquire in the course of their economic activity, and if any other change of knowledge must be regarded as a " change in the data " in the usual sense of the term, which falls outside the sphere of equilibrium analysis, this would mean that equilibrium analysis can really tell us nothing about the significance of such changes in knowledge, and would go far to account for the fact that pure analysis seems to have so extraordinarily little to say about institutions, such as the press, the purpose of which is to communicate knowledge. And it might even explain why the preoccupation with pure analysis should so frequently create a peculiar blindness to the rôle played in real life by such institutions as advertising.

X

With these rather desultory remarks on topics which would deserve much more careful examination I must conclude my survey of these problems. There are only one or two further remarks which I want to add.

One is that, in stressing the nature of the empirical propositions of which we must make use if the formal apparatus of equilibrium analysis is to serve for an explanation of the real world, and in emphasising that the propositions about how people will learn, which are relevant in this connection, are of a fundamentally different nature from those of formal analysis, I do not mean to suggest that there opens here and now a wide field for empirical research. I very much doubt whether such investigation would teach us anything new. The important point is rather that we should become clear about what the questions of fact are on which the applicability of our argument to the real world depends, or, to put the same thing in other words, at what point our argument, when it is applied to phenomena of the real world, becomes subject to verification.

The second point is that I do not want of course to suggest that the sort of problems I have been discussing were foreign to the arguments of the economists of the older generations. The only objection that can be made against them is that they have so mixed up the two sorts of propositions, the *a priori* and the empirical, of which every realistic economist makes constant use, that it is frequently quite impossible to see what sort of validity they claimed for a particular statement. More recent work has been freer from this fault—but only at the price of leaving more and more obscure what sort of relevance their arguments had to the phenomena of the real world. All I have tried to do has been to find the way back to the common-sense meaning of our analysis, of which, I am afraid, we are apt to lose sight as our analysis becomes more elaborate. You may even feel that most of what I have said has been commonplace. But from time to time it is probably necessary to detach oneself from the technicalities of the argument and to ask quite naïvely what it is all about. If I have only shown that in some respects the answer to this question is not only not obvious, but that occasionally we do not even quite know what it is, I have succeeded in my purpose.

The Pig-Cycle in Great Britain: An Explanation

By R. H. Coase and R. F. Fowler

Modern economic theory is laying increasing emphasis on the importance of considering the nature of producers' anticipations. There is, however, comparatively little statistical analysis of the way in which producers do, in fact, forecast. In connection with one industry, that of pig production, some study of the problem has been made, but, unfortunately, as we showed in a previous paper,[1] the conclusion to which these investigations have led seems to be untenable. The view which had been commonly held by agricultural economists was that farmers assume that present prices and costs will continue unchanged in the future. This assumption we showed to be incorrect, a conclusion with which Miss Cohen and Mr. Barker[2] were in agreement.[3] They objected because we had " in effect, tried to overthrow the existing explanation without putting anything in its place ". In fact, however, such explanation as they attempted in their Reply involved certain important modifications of the theory previously held and was not based on any analysis of the way farmers had behaved but was purely hypothetical. It followed that the old theory had been abandoned while the one put forward in its place was not based on any investigation of observed facts. Such tests of this new hypothesis as we were able to make indicated that it did not seem to fit the facts and we concluded that " a theory of the pig-cycle is still lacking ". In this paper, we make a further investigation and put forward an explanation of the pig-cycle which we believe to be more in accordance with the observed facts.

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[1] " Bacon Production and the Pig-Cycle in Great Britain," Economica, May, 1935.

[2] " The Pig-Cycle: A Reply," Economica, November, 1935.

[3] In spite of this, they affirm the essential correctness of the views of those who uphold the " cobweb theorem ". We are unable to understand their contention that " the explanation of the pig-cycle put forward by the Re-organisation Commission is essentially correct ". (P. 422). If pig producers do not assume that present prices and costs will continue unchanged in the future, then *the " cobweb theorem " does not apply to British pig farming*. This was what we set out to show. Their modification, with which, of course, our original paper was not concerned, we examine below and show to be incorrect.

I

We may commence by a description of the structure of the British pig industry. Pig farmers in Great Britain produce two types of pig: porkers and baconers. Porkers are normally slaughtered at about 4 months, while baconers are 7 to 8 months old at slaughter. About 70 per cent. of the pigs produced during the period 1921–1933 were pork pigs. Some farmers specialise on the production of porkers or baconers, but most farmers do not. One form of specialisation should, however, be noted, that between breeders and feeders. Breeders produce store pigs which are sold at from eight to sixteen weeks to feeders. These store pigs will then be fed for the pork or bacon markets. Some porkers or baconers will not be sold in the store market but will be fattened by the breeder. This specialisation is, however, stated by Mr. Calder[1] to be "widespread".

The price statistics with which we are concerned will therefore be those relating to stores, porkers and baconers. In our previous paper we concerned ourselves solely with the production of baconers and we took changes in the total production of pigs as being indicative of changes in the production of baconers. We noted that there was a considerable liability to error in so doing. However, an analysis of the profits received from the sale of porkers reveals that the fluctuations accord very closely with those from the sale of store pigs. It follows therefore that in using, as we did in our previous paper, the "profits" margin of breeders of store pigs as an index of the profitability of pig production, we were in fact taking an index which applied to 80 or 90 per cent. of the total production of pigs.[2] In this paper, we shall be concerned not solely with the production of bacon pigs, but with the production of pigs in general.

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[1] Calder: "The Development of the British Pig Industry." *The Pig Breeders' Annual*, 1932–1933. If the figures for 41 representative markets published in the Agricultural Marketing Reports are really representative, though of this there seems to be some doubt, it can be concluded that two-thirds of the pigs sold at markets go through the hands of breeders and feeders. Some indication of the magnitude of this differentiation can be obtained from an investigation made in Northern Ireland. This showed that 48 per cent of pig farmers were feeders only. See the Monthly Report for August, 1936, of the Government of Northern Ireland, Ministry of Agriculture.

[2] This figure will, of course, tend to be higher the greater the specialisation between breeders and feeders.

The pig-cycle in Great Britain is usually stated to be one of four years, two years of rising prices being followed by two years of falling prices.[1] The prices of porkers, baconers and store pigs all show the same general movements, but those of porkers and store pigs, which accord more closely, seem to show a tendency to lag behind the movements of baconer prices. In order to solve the problem of the pig cycle, we have to explain two things: firstly, why there is a cycle at all, and secondly, the length of the cycle.

II

The problem of explaining why there is a cycle, given the technical facts of production,[2] is essentially one of determining the nature of the forecasts made by pig farmers. In our previous article we rejected the view that farmers assume that present prices and costs will continue unchanged in the future. Our principal objection was based on a statistical analysis of the behaviour of feeders. We argued that since the store market was competitive, the price of store pigs would be bid up or down in such a way as to make the difference between the expected future price of a baconer and the sum of the cost of feed for five months and the present price of a store pig equal to the "normal" profit margin. If we assume that this "normal" profit margin remains constant, it is possible to calculate what feeders expected the price would be.[3] We concluded, and Miss Cohen and Mr. Barker agreed, that feeders do not expect present prices and costs to remain unchanged. We worked out our result in terms of "normal" profits while Miss Cohen and Mr. Barker calculated in terms of the expected price.[4] It is, of course, another way of looking at the same thing. It is perhaps more convenient to use the latter method and in this paper we shall consider the expected price rather than deviations from the "normal" profit margin.

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[1] See, for example, the *Report of the Reorganisation Commission for Pigs and Pig Products*, p. 13.

[2] The theory necessarily makes certain assumptions relating to the elasticity of demand, the elasticity of supply and to certain differences between long and short-run elasticities of supply. See Coase and Fowler, op. cit.

[3] If A is the expected price of a baconer, B is the price of a store pig, C is the cost of feeding and D is the "normal" profit, then A is equal to B plus C plus D.

[4] See Cohen and Barker, op. cit.

Since the old explanation of the pig-cycle has to be rejected, what other possible assumptions can be made about the nature of farmers' forecasts ? Five possible assumptions can be distinguished.[1]

1. Farmers assume[2] that future prices will bear some definite relation to present prices.[3] Thus, future prices are estimated to be x per cent. of present prices—x being unchanged except that it may be different when prices are rising from what it is when they are falling.[4]

2. Farmers assume that future prices will change at a rate which is greater than, equal to, or less than the rate at which present prices are changing.

3. Farmers assume that future prices will bear a definite relation to present prices, but modified according to the rate of change of existing prices. This is a combination of 1 and 2.

4. Farmers assume that future prices will bear a definite relation to an average of past prices. This involves a delayed reaction on the part of farmers ; in other words, they react only to changes over a period.

5. Farmers do not make any assumptions of such a definite nature that they can be determined in a simple form from existing or past prices and costs.

We will now examine these different assumptions.

1. This assumption suggested by Miss Cohen and Mr. Barker in their Reply was rejected by us in our Rejoinder as it did not accord with the facts. This can perhaps best be shown by the use of scatter diagrams. For this purpose we have considered the nine-year period 1922 to 1930. Diagram I shows the relationship between actual and expected prices of a nine-score bacon pig when actual prices are rising. Diagram II shows the same relationship when prices are falling. By rising prices we mean that the actual price is higher than that existing in the previous month and *vice versa* for falling prices. The diagrams show quite clearly that there is no relationship of the type suggested.

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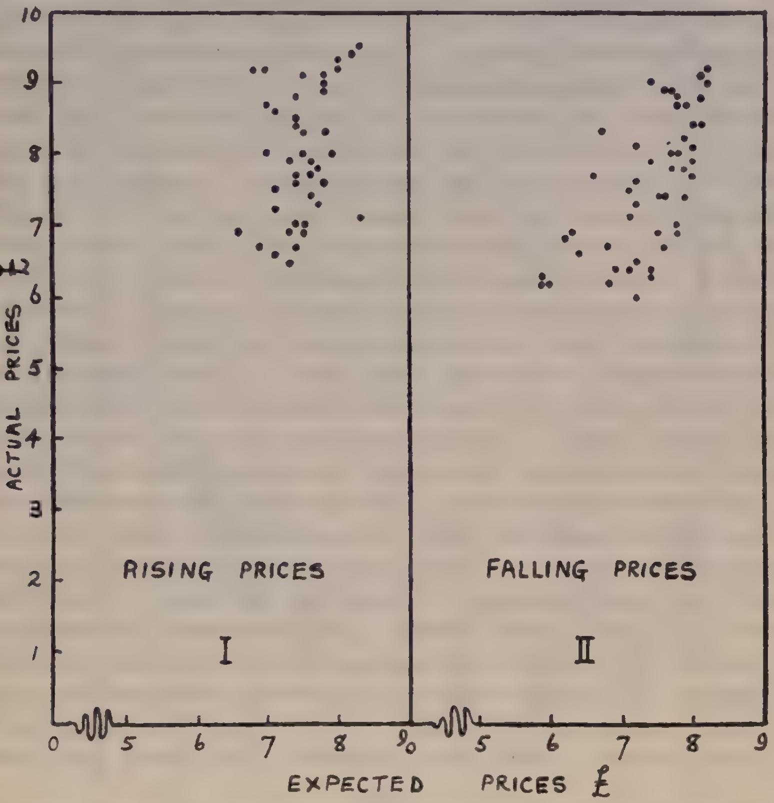
[1] Of course, other assumptions about farmers' forecasts could be made. But if they are of the rigid nature generally assumed, it would seem probable that the assumption made would be one of the first four mentioned.

[2] When we speak of farmers' assumptions about future prices being of a certain nature, all we mean is that farmers act as if they make these assumptions.

[3] The strict " cobweb theorem " is a special case where x is equal to 100 per cent. Miss Cohen and Mr. Barker put forward this modification of the " cobweb theorem " in their Reply.

[4] The fact that x may be different when prices are falling or rising is a modification we have introduced to give this suggestion greater generality.



DIAGRAMS I AND II. The Relationship between the Actual and Expected Prices of a 9-score Bacon Pig.

Naturally, one would expect that when actual prices are low, expected prices would be low; but even this is not very evident from the diagrams. The range of expected prices for any given actual price or *vice versa* is much too great to enable us to postulate the existence of such a relationship.

2. It is important to keep in mind the fact that the expected value of a baconer fluctuates less than the actual value. No assumption about farmers' forecasts can have any validity unless it gives this result. Let us first consider the case where farmers assume that the present rate of change of price will be maintained during the next five months. Suppose first of all that there is a cycle of prices with a constant amplitude and without any secular trend. The rate of change of price may be calculated by subtracting the price in one month from the price in another

and dividing by the number of months which has elapsed in the intervening period. Since we have assumed that the cycle has a regular amplitude, it can be shown that if farmers assume that the present rate of change of prices (calculated over any period) will be maintained, then expected prices will fluctuate more than actual prices. Suppose that the actual price in a particular month is a peak price. The rate of change of price (whatever past price other than a peak price is taken as base) will necessarily be positive because all other prices are less than the peak price.[1] Therefore, the expected price five months after the present peak price would be higher than this peak price. That is, fluctuations in the expected price would be greater than fluctuations in the actual price. Our assumption was, of course, that there was no secular trend, and this was approximately true of the period 1922 to 1929. We can therefore state definitely that this assumption that farmers believe that the rate of change of price will continue unchanged is incompatible with the observed facts. But even for those years when there was some secular trend, *e.g.* 1930 onwards, a statistical analysis proves this assumption to be without validity. Diagram III shows what the expected price would be on certain assumptions relating to the period considered if farmers acted in the way postulated, and shows that none

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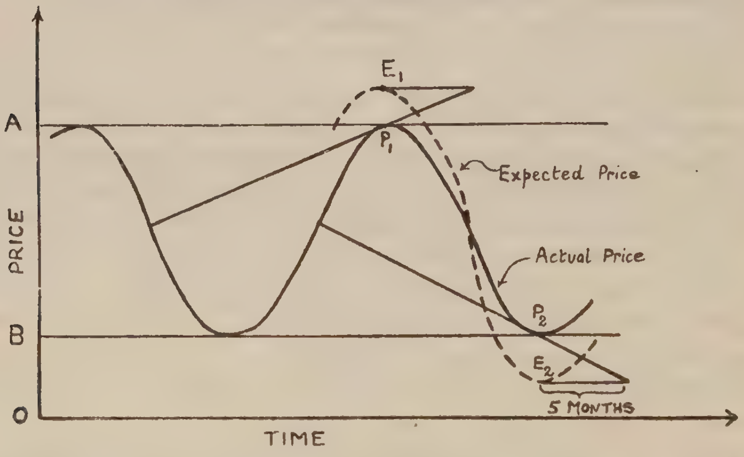

Let OA be the peak price and OB the depression price. Let the present price be a peak price P_1. Consider now the rate of change between P_1 and any preceding price other than a peak price OA. Then this rate of change must clearly be positive. The price which farmers at this point of time would expect if the present rate of change were to continue is E_1 and must obviously be greater than OA. Similarly, it can be shown that E_2 is less than OB.

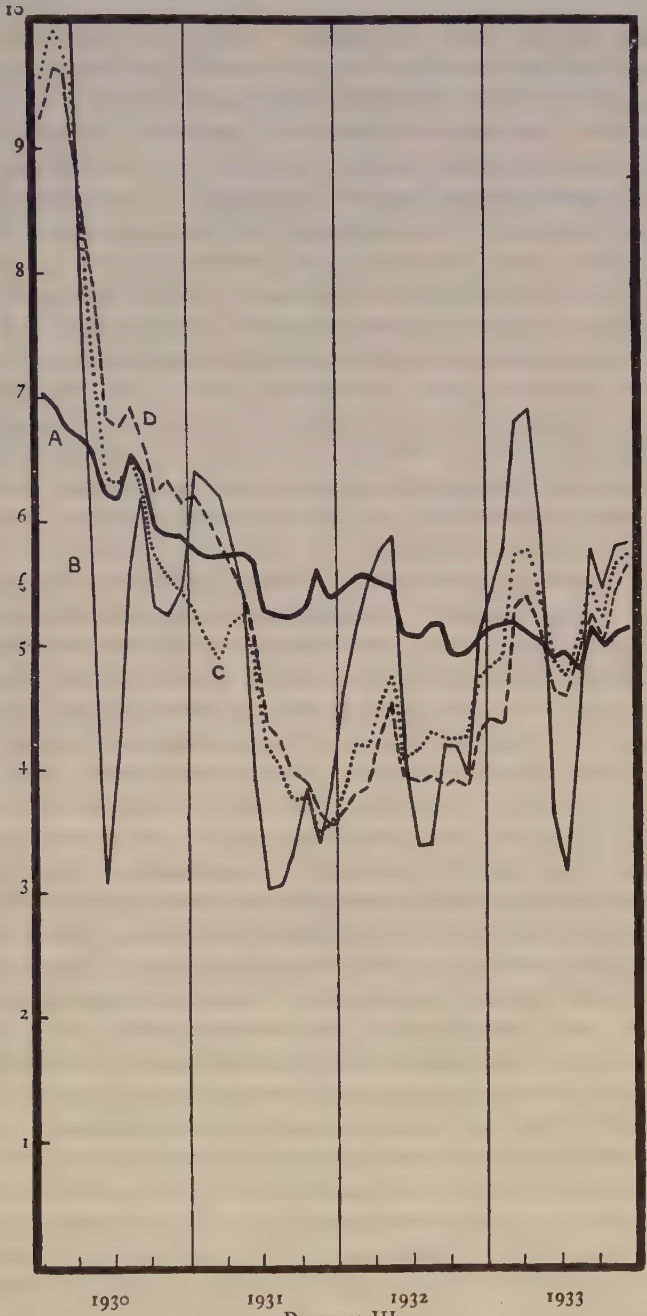

DIAGRAM III.

(A) Expected Price of Bacon Pig.

(B), (C), (D) Expected prices if farmers believe that the rate of change of price over the previous 3 months (B); 1 year (C); and 2 years (D) is maintained.

of these curves is in agreement with the price actually expected.[1]

We have now to consider those cases in which farmers assume that the rate of change of price will be greater or less than the previous rate of change. In the former case, our previous argument applies *a fortiori*. Even in the latter case our conclusion is not affected so long as farmers expect prices which have been rising to continue to rise and prices which have been falling to continue to fall. Naturally, the fluctuations in the expected price will be lessened, but they will still be greater than those of the actual price. The fact is, as we pointed out in our earlier paper, that farmers act as if they expected prices to fall when they are approaching the peak price and to rise when they are approaching low price levels. This implies that they expect price to move in a contrary direction at these times to that of existing prices.

3. We have shown that if one considers the relation between expected prices and either actual prices or the rate of change of price, no definite relationship can be obtained. It is possible, however, that for any given actual price the expected price will be greater or less according as the rate of change of price is greater or less. That is, it may well be that the expected price which corresponds with any actual price will be different with different rates of change of price. This is the possibility which we now have to examine. The number of cases is insufficient to enable us to find out whether this is so for any *one* actual price. We can, however, consider a range of actual prices, *e.g.* those falling between £6.5 and £7.5. This we have done. The number of cases is still not very great, but if there were any correlation it might well be expected to manifest itself. The table which follows distinguishes three ranges of actual prices and shows for each range the average expected price which corresponds with a given average rate of change.

It should be noted that the rate of change is calculated over a period of two months, that the table refers to rising prices and that only those cases have been taken where the rise in price was continuous. The number of expected prices of which the figure given is an average is shown in brackets.

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[1] The curve showing the expected price is probably at too high a level owing to the fall in profits in alternative occupations. But this would not affect our conclusion.

TABLE I

AVERAGE OF EXPECTED PRICES ASSOCIATED WITH CERTAIN RANGES OF ACTUAL PRICES AND RATES OF CHANGE OF PRICE

Rate of change of price over two months.	Actual Price		
	£6.5 to £7.5	£7.5 to £8.5	£8.5 to £9.5
Up to £0.4	7.54 (7)	7.58 (2)	7.71 (4)
£0.4 to £0.7	7.47 (6)	7.53 (2)	7.62 (2)
Over £0.7	7.06 (1)	7.64 (3)	7.66 (3)

If there were any relationship one might imagine that the average expected price would be higher when the rate of change of price is higher. No definite relationship of any sort is, however, discernable. Though the rate of change has been calculated only for one period, it does not appear as if any correlation would be obtained were any other period taken. Until further information is available, therefore, this assumption has to be rejected.

4. This assumption, another modification of the " cobweb theorem ", might at first sight appear to be a reasonable one. Two cases can be distinguished. The first is where each price in the past is given an equal weight. Different periods can, of course, be taken. The second is where weights are given to the different prices. Since it is most unreasonable to assume that past prices have the same significance as recent prices, this would involve giving greater weights to present than to past prices. Unweighted averages for different periods are shown in Diagram IV. It is clear that none of these corresponds with the price actually expected. In general, the curves corresponding with the longer periods are closer to the curve showing the expected price. The correspondence between the three-year curve and the curve of expected prices, for instance, in 1925 and 1926, is very close. But at other times, for instance, in 1931, the curves for the shorter periods correspond more closely. Weighting more recent prices would not seem to improve the fit ; on the whole, it would probably worsen it. We can therefore conclude that this view about the behaviour of pig farmers would be untenable.

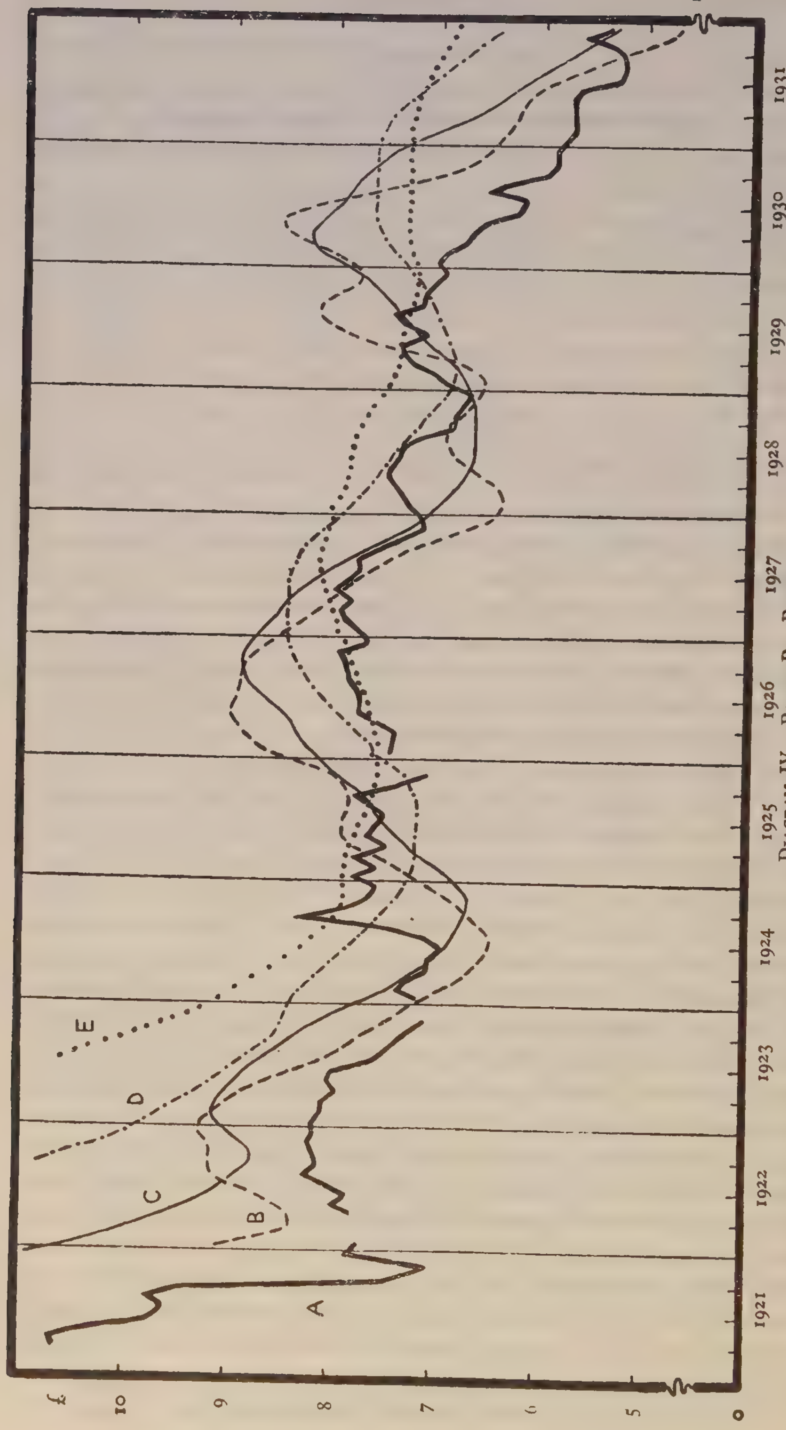

DIAGRAM IV. BACON-PIG PRICES.

(A) Expected Price. (B), (C), (D), (E) Unweighted Average of previous 6 months, 12 months, 24 months and 36 months respectively.

5. This would seem to be much the most reasonable assumption. A discussion of the general influences which affect farmers in making their forecasts, is, however, deferred until certain other aspects of the problem have been elucidated.

Our discussion so far has been based on an examination of the reactions of feeders. It would be appropriate now to point out that the lag between the prices of baconers and the prices of store pigs which previously we noted as a "curious phenomenon" can be explained by the use of Curve A in Diagram III of our original article. This curve showed what the "profits" of feeders would be if present prices and costs remained unchanged. If at any time this "profits" margin was above or below normal, then this indicated that feeders did not assume that present prices and costs would remain unchanged, but that the "profit" margin would become unfavourable or favourable. We have to compare this graph with one showing store pig and baconer prices. As an example, we can take the period 1925 and 1926. During the latter half of 1925, the price of baconers rose steeply. The price of store pigs, however, did not rise sufficiently to reduce the hypothetical margin to normal and in fact, actual "profits" rose considerably. Then in January, 1926, baconer prices started to fall. Store-pig prices, however, continued to rise. The average price of a first quality store pig which had been £2.57 in January had risen to £3.04 in September. Why did feeders bid up the price of store pigs at a time when baconer prices were falling? Although baconer prices were falling, the relation of store-pig prices to baconer prices was such that "profits" had been above normal and would continue to be if this relation remained unchanged. This being so, there would be an incentive for feeders to bid up the price of store-pigs, since the profitability of feeding was above normal. This phenomenon, which at first sight appeared so curious is probably due to the fact that the initial position was one not of equilibrium but of disequilibrium. Any tendency to equilibrium, therefore, would show itself in a rise in store-pig prices, even though baconer prices were falling.

The analysis of the actions of feeders, considered by examining the relationship between actual and expected prices, has not led to any positive result. We must therefore turn to an examination of breeders' reactions. Breeders' reactions to changes in prices and costs will show themselves in their attitude to the slaughter of old sows and also in their attitude to new investment in gilts.[1] No figures are published of the number of new gilts which have been introduced, but it is possible to obtain an estimate by calculations based on the figures relating to the total sow population.

If the sow population has been stationary for some period, the number of sows in one age group is equal to the number in any other group. Now suppose that the length of life of a sow is on the average x years. Then we know that in any year, the number of sows which have died or been slaughtered is 1/x of the sow population, and this will be equal to the number of new gilts introduced, since the total sow population remains constant. Let us now suppose that the sow population which has been stationary increases. Then we know that the number of gilts introduced during the year was equal to 1/x of the sow population *plus* the difference between the sow population in the two years. Since the sow population is increasing, the number of sows which die or are slaughtered will decline as a proportion of the total sow population. If this proportion is known we can calculate the number of sows which will be discarded during the year and can thus estimate the number of new sows introduced. We do not, however, know what the age distribution of the actual sow population is; nor will the age distribution be that appropriate to a stationary population. We are considering in detail in this paper the period 1921 to 1931 and have therefore taken as our starting point the first post-war year 1919. We have assumed that the age distribution of the sow population in that year is as follows:[2]

Age.	*Per cent. of total sow population.*
6 months–1 year	20
1 year–2 years	60
2 years–3 years	20

It is also necessary to determine the average age at slaughter of a brood sow. No published statistics are available regarding this point. In *Pig-keeping*, issued by the Ministry of Agriculture, it is stated that " It is a common opinion that sows should be discarded for breeding after their fourth litter, but provided the sow is breeding well, it would seem advisable to breed from her as long as she will produce profitable litters".[1] If the sow farrows first at one year and has two litters a year, sows slaughtered after four litters would be two and a half years old at slaughter. There seems to be some doubt as to the average age at slaughter, two and a half years being considered by some to be too low a figure. A reasonable assumption would appear to be that, taking into account the losses through accident and disease and the culling of unsatisfactory sows, the average life of a brood sow is 3 years. Small differences from this figure are not likely to alter greatly our result. The figures published by the Ministry of Agriculture of the number of breeding sows refer to "sows kept for breeding and gilts-in-pig", and we are informed that gilts under 6 or 8 months are almost certainly excluded. Statistically therefore the average age of a brood sow at death or slaughter is 3 years minus 6 months, that is, 2½ years, and we have therefore used this figure. It would be much better to take the proportion of sows which die at different ages instead of assuming, as we do, that all pigs die at the average age. Unfortunately, no information is at present available which would enable us to make this calculation. Our results are therefore subject to this error. So long, however, as the average age is correct, this is not likely to be so great as to vitiate our conclusions. Our estimate of the input of gilts is shown in Column I of Table II, on the next page.[2]

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[1] A gilt is a young sow.

[2] This distribution can be considered as reasonable having regard to the movement of the sow population in the previous few years. Experiment shows that changes in the assumed age distribution will not alter very greatly our result.

During the period 1921–1930, there was hardly any trend in either prices or the production of pigs in Great Britain. There was probably an increase in the demand for bacon which was satisfied by increased supplies from abroad, principally Denmark.[3] We can assume therefore that if the pig population had remained stationary then the price would have been constant (apart from seasonal movements) and equal to the average of prices during this period. The average number of sows used for breeding between 1921 and 1931 was 369,000. We can therefore assume that this

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[1] Ministry of Agriculture and Fisheries, Bulletin No. 32, *Pig-keeping*, p. 11.

[2] There is, unfortunately, little information which can be used to check the accuracy of our results. Such evidence as there is, for instance, the Reports of the East Anglian Pig Recording Scheme, issued by the University of Cambridge, would seem to support our result. But the sample considered is small and it would seem desirable that an attempt should be made to discover the input of gilts in a more direct way than we have been able to employ.

[3] See Coase and Fowler, op. cit., p. 161.

Table II

June to June.	Input of Gilts. (A)	Input required to bring sow population to " normal " of 369,000 (B)	Difference. (A) — (B)
	Thousands	Thousands	Thousands
1919–1920	91	—	—
1920–1921	210	225	− 15
1921–1922	65	113	− 48
1922–1923	243	199	+ 44
1923–1924	197	93	+ 104
1924–1925	14	50	− 36
1925–1926	206	256	− 50
1926–1927	207	156	+ 51
1927–1928	92	59	+ 33
1928–1929	127	173	− 46
1929–1930	161	196	− 35
1930–1931	198	—	—
1931–1932	166	—	—
1932–1933	163	—	—

is the " equilibrium " sow population. In Column 2 of Table II is shown the number of new gilts required each year to bring the sow population up to this figure, and Column 3 shows the amount by which the actual input exceeded or fell short of the amount required to bring the sow population to " normal ". The figures in columns 1 and 2 are displayed graphically in Diagram V as Curves A and B. Curve C shows the actual price of a nine-score bacon pig while Curve D shows what feeders appear to have expected the price to be 5 months afterwards. Curve E of Diagram V represents the " profit " margin received on the sale of a pork pig.

We are now in a position to analyse the reactions of breeders during the course of the pig-cycle in the period 1921 to 1930.

1921–1922.

Prices fell during 1921 to a minimum at the end of the year and then began to rise during 1922, and this movement was reflected in " profits ". Feeders seem to have believed that prices were going to fall. Of course, breeders may not be of the same mind, but the fact that the input fell and was below that necessary to bring the sow population to " normal " suggests that in fact breeders also thought prices were going to fall.

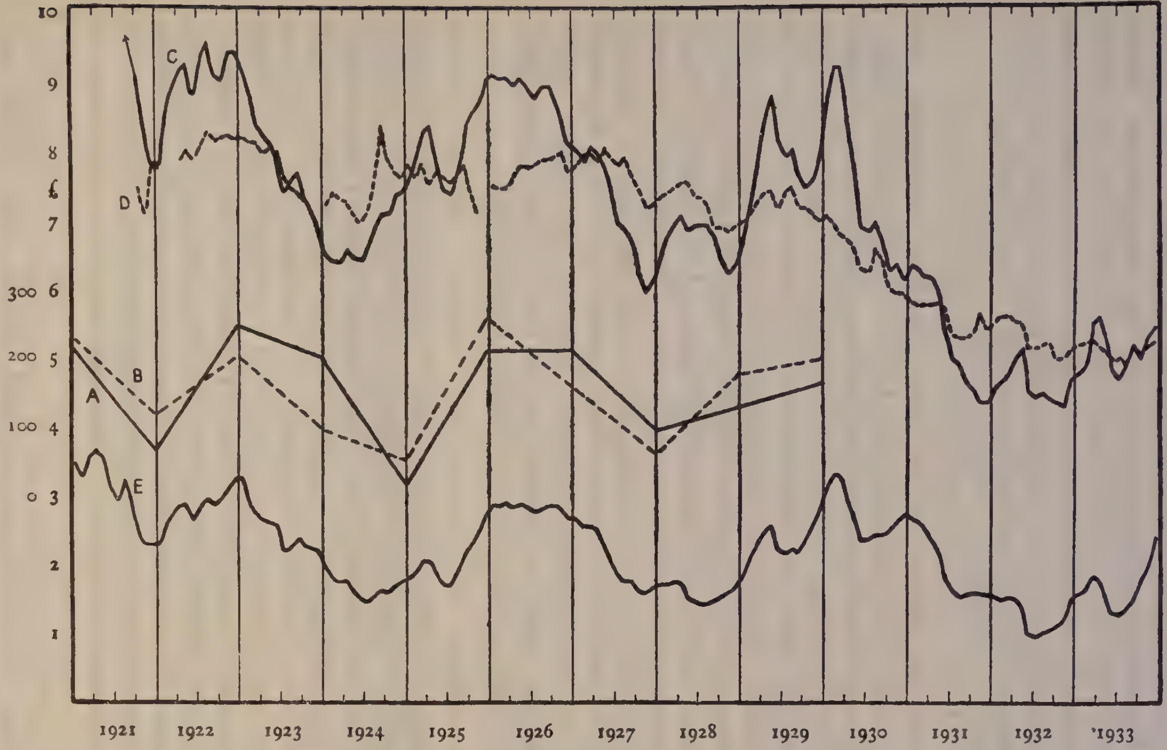

DIAGRAM V.

(A) Estimated Input of Gilts each year, in thousands. (B) Input of Gilts required each year to bring the sow population to normal of 369,000. (C) Average Monthly price of a 9-score deadweight Bacon Pig. (D) Expected value of a Bacon Pig. (E) " Profits " Margin (receipts less cost of feed) per Pork Pig (4-score deadweight).

1922–1923.

Between June 1922 and June 1923, the "profit" margin remained above normal and feeders over most of the period seem to have expected prices to fall but remain above normal. The same view again seems to have been held by breeders, who increased the input of gilts from 65,000 to 243,000, which was, however, only 44,000 more than was necessary to bring the sow population up to normal.[1]

1923–1924.

During the latter part of 1923, feeders considered that prices would continue unchanged, but in 1924, they thought prices would rise from the low level to which they had fallen. Breeders' "profits," which had been about normal towards the end of 1923, fell below normal in 1924. The input of gilts during this period, although below that of the previous year, was none the less considerably above the level necessary to bring the sow population to normal. The estimated input was in fact 197,000 instead of 93,000. Feeders seem to have thought that prices would rise but not above normal. Breeders, however, seem to have acted as if they expected a rise of prices above normal. This is, of course, quite inconsistent with the assumption of the "cobweb theorem" that present prices would continue unchanged.

1924–1925.

Throughout this period "profits" remained below normal. In spite of rapid price changes, feeders expected bacon pig prices to be normal. The only month when this was not true was September, when for some reason prices were expected to rise above normal. New input of gilts was reduced considerably to 14,000, but it should be noted that the number of new gilts required to bring the sow population to normal was only 50,000. Breeders reduced the size of their herds by more than the amount required to maintain a normal sow population of 369,000, but the error was not excessive when compared with errors made in other years.

1925–1926.

During the latter part of 1925, prices rose rapidly and remained constant at a high level during 1926. "Profits" were above normal, except at the very beginning of the period. However, feeders seem to have expected prices to fall, and breeders, although increasing the input of gilts, did not expand the size of their herds sufficiently to bring the sow population to normal. Again, we find the reactions of breeders and feeders in accord. If the " cobweb theorem " were applicable, one would have expected the exact reverse to have happened and for the expansion to have been greater than was necessary to bring the sow population to normal. This would have been technically possible.[1]

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[1] It should be noted that the difference between our estimate of the actual input and the input required to bring the sow population to "normal" is not affected by changes in the assumptions made regarding the length of life of a brood sow.

1926–1927.

Baconer prices began to fall from September, but porker prices remained fairly steady until January, 1927, when they too began to fall. " Profits " remained high and above normal though tending to fall. Feeders during the latter part of 1926 expected prices to fall, but in 1927 they expected prices to remain at just under £8, the level existing at the beginning of the year. The new input of gilts brought the sow population above normal.

1927–1928.

Prices continued to fall during 1927, but showed some rise at the beginning of 1928, although remaining below normal, as did " profits " throughout this period. New input, however, although falling, was above the amount necessary to bring the sow population to 369,000. Feeders expected prices to rise but breeders appear to have expected an even greater price rise.

1928–1929.

Porker prices rose until May but baconer prices fell towards the end of 1928 and then rose rapidly to a peak in May. " Profits " rose until May and then fell slightly. On the whole, they were below normal. Feeders expected prices to rise during the first part of this period and to fall during the latter part, the average expected price being below normal at about £7. This attitude seems to have been similar to that of the breeders, the estimated input of gilts being 127,000 instead of the 173,000 necessary to bring the sow population to the " equilibrium " number.

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[1] See Calder, op. cit.

1929–1930.

Prices and "profits" were both at high levels during this period, a peak being reached in May. In spite of this, the input of gilts by breeders was below the amount necessary to bring the sow population to the normal of 369,000. This would indicate that breeders expected prices to fall below normal. Feeders also seem to have expected prices to fall.

A noteworthy point arising out of this analysis is the agreement which there seems to be between the conclusions drawn from a consideration of feeders' reactions and those obtained from an investigation of breeders' reactions, obtained by estimating their new investment in sows. It would seem to increase the probability that conclusions drawn from an analysis of feeders' reactions are correct in those cases where confirmation by reference to breeders' policy is not possible.

When, however, an attempt is made to summarise this detailed analysis of farmers' reactions during the period 1921 to 1930, a difficulty immediately arises. During this period, farmers' reactions do not appear to have been uniform at the same phase of successive pig-cycles; a result which is highly significant when it is realised that only a nine-year period has been considered. This lack of uniformity in the behaviour of farmers is striking. If we consider those cases when farmers increased new investment to a point where the sow population was above the "equilibrium" number, when, that is, farmers expected prices to be above normal,[1] we find this view held twice in periods of high prices and profits, once in a period of low prices and profits, while one case it is difficult to classify. The view that prices would be below normal was held twice in periods of low prices and profits, twice in periods of high prices and profits, while, again, there is one case which cannot be classified under either of these two heads.

Since it is impossible to link up farmers' reactions in any definite way with existing prices, and, as we showed in connection with feeders, any modification of the "cobweb theorem" to include past prices does not seem to throw light on this problem, it seems necessary to fall back on the fifth assumption which we mentioned above. This seems to be the only view which, on the evidence available, can be held. Farmers' assumptions are not of such a definite nature that they can be determined from existing or past prices and costs. Naturally, we cannot expect them to be accurate in their forecasts.[1] Errors will necessarily occur and since the demand for pig products is probably inelastic, the fluctuations in prices and profits will be great. But if this is the reason for the fluctuations in price which do occur, why is there a four-year cycle ? Why is it that farmers apparently for two years in succession think prices are going to be above normal and then for two years think they are going to be below normal ? We have therefore to examine the problem of the length of the cycle.

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[1] We are not, of course, assuming that *all* farmers entertain the same views about future prices. If the output produced is the one appropriate to a price less than the existing one, then we think we assume that farmers *on the whole* expected prices to fall and vice versa if the output is greater.

III

In our previous paper, we stated that if the " cobweb theorem " did apply to the British pig industry, then the cycle would be one of two years. Miss Cohen and Mr. Barker objected to this and tried to show that a four-year cycle was consistent with the assumptions of the " cobweb theorem " since " it will pay to go on breeding from sows which have already had from one to five litters, at a price at which it would not pay to breed from gilts for the first time ". We agree that if there is no undercapacity in brood sows this will be a factor tending to lengthen the cycle. We shall try to show, however, that the cycle is in any case considerably shorter than four years. Furthermore an explanation of the length of the cycle based on the fact mentioned by Miss Cohen and Mr. Barker implies, as we shall show, that the cycle will have characteristics different from those which we actually observe in the British pig industry.

We shall first of all consider what the length of the cycle would be *if the assumptions of the " cobweb theorem " were correct.* Let us consider the reactions of producers of porkers and store pigs. According to Mr. Calder,[1] the technical factors are such that expansion must start by retaining an increasing number of gilts, the most advanced of which would be approaching the bacon weight at 6 to 7½ months of age. With a 2 months lapse prior to service, 4 months gestation and 4 months to bring litters to porker weights, it would take at least 10 months following the initial increase in breeding stock to bring about a material increase in the number of porkers. By retaining the best gilts at the rate of one in every six produced, the number of breeding stock could be doubled in one year.[1]

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[1] We do not agree with the view expressed by Professor Hayek in his *Monetary Theory and the Trade Cycle*, that if a large number of producers, each with his own views on what the future price will be, act independently, then errors of optimism and pessimism will cancel out and the equilibrium output will be produced. It is a most improbable result. Almost certainly there will be an error in one direction or the other, either too much or too little being produced. Cf. P. N. Rosenstein-Rodan, " The Coordination of the General Theories of Money and Price," ECONOMICA, August, 1936.

Let us assume that prices are rising and that they have just reached normal. It follows that eight months previously the sow population was normal. If no new sows had been introduced the sow population would have been 7/9ths of normal when prices had become normal.[2] But we cannot assume that no new sows are introduced during this period, in spite of the fact that prices are below normal. If the supply were infinitely elastic at the normal price no new sows would be introduced until the price became normal. However, since the elasticity of supply is not infinite there will be a definite number of sows corresponding with each price. When prices are below normal and the sow population is normal there will be no incentive for new gilts to be introduced. But after this point prices are rising and the sow population falling as a result of death and normal culling. At some point, therefore, before the normal price is reached, there will be an incentive to introduce new gilts. As prices are rising and farmers assume that existing prices will continue, farmers will endeavour to increase the sow population.[3] Since there is a definite number of sows corresponding with each price and prices are rising, the sow population appropriate to each price which farmers (according to the " cobweb theorem ") assume to remain constant will increase as prices increase and will be equal to normal when prices are normal. Prices will start to fall ten months after the initial introduction of new gilts, and if farmers are able to make the sow population normal when prices reach normal, prices will again be normal in ten months time. We have to consider, however, whether this is technically possible. Assuming no seasonal variations in the elasticity of supply, it can be shown that a pig population which has been allowed to decrease from normal can be brought back to this level within eight months provided that the expansion starts a little more than two months before the end of the period.[1] The actual period of time which will elapse before new gilts are introduced will be greater the more elastic the supply curve and the more slowly prices rise at the beginning of this period. It is not possible, therefore, to state exactly when farmers will start to introduce new gilts. Ten months from this point of time, however, prices will start to fall. Up to this peak price, farmers will be increasing the sow population. When prices fall, since the fall will be rapid and the rate of depreciation of the sow population is small, it is probable that no gilts will be introduced after the peak price. The sow population although declining will always be greater than that appropriate to the existing price. Prices will therefore start to rise 10 months after the peak price. What we have to determine is when prices reach normal again. To determine this exactly we would have to know the size and age composition of the sow population at the peak price. This will depend on the elasticity of supply, the time which elapses between normal prices and the peak price and whether the sow population is normal when prices are normal. We do know, however, that the maximum increase in the sow population from one year to another is about 30 per cent. If the "cobweb theorem" is an explanation of the pig cycle in Great Britain, then we may use this fact as an indication of the increase which might well be expected during the period of a year. Now the period during which prices rise from normal to the peak may be anything up to 10 months. It should be noted, however, that if it is 8 months or more, then the initial sow population will be one below normal. This will be a factor tending to reduce the size of the sow population at the peak price. If we assume, however, that the sow population is normal when prices are normal, then the period during which prices rise from normal to their peak will be a maximum of 8 months. If we take 30 per cent. to be the appropriate increase over the year, then we can take 20 per cent. as being the appropriate increase over a period of 8 months. At the peak price, therefore, the sow population will be 120 per cent. of normal. Clearly even if the rate of depreciation of the sow population is half that of normal the sow population will be reduced to 100 within a year. Of course, as we have already shown, if at any time no input of gilts had taken place, the sow population would have been reduced below the average in considerably less than a year. We may conclude, therefore, that the sow population will be reduced to normal within a year at most, and prices will reach normal eight months afterwards. This analysis would therefore appear to give a cycle of between 18 and 30 months, the most probable figure being two years. It should be noted that the period during which prices are falling is always 10 months and that if the cycle is lengthened by the increase in the sow population being more than the assumed 20 per cent., the asymmetry is increased. This is quite contrary to the observed facts relating to the cycle, which is roughly symmetrical.

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[1] Calder, op. cit.

[2] Assuming one-third of the sows are slaughtered or die each year, then at the end of a year (starting from the normal population), the sow population would be two-thirds of normal; at the end of eight months it would be seven-ninths of normal.

[3] The assumption that the sow population increases as new gilts are introduced implies that no herds are larger than the size appropriate to the existing price. The error, however, is likely to be small.

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[1] This is based on Mr. Calder's assumption that only one gilt in six is used for breeding. The number could, of course, be increased (though worse gilts would have to be used) and to the extent that this is done, the time required will be reduced.

One point we have ignored is the possibility of changing the length of life of a brood sow. This will operate by increasing the number of sows at any point of time in periods of high prices and vice versa in periods of low prices. Prices will therefore fall faster than has already been suggested. The effect will be to reduce the length of the cycle. There are also certain factors which might operate to increase the length of the cycle. The length of the cycle will be affected by the seasonal fluctuations in farrowings,[1] for if the decision to increase production is made in any season other than the spring, expansion is likely to be rendered more difficult and the length of the cycle increased. Secondly, we have assumed that all pigs were sold in the pork market. Since, however, 30 per cent. of British pigs were sold in the bacon market, this would tend to increase the length of the cycle.[2]

It should be noted that the periods of low prices are much longer than the periods of high prices. If, however, average prices are less than normal prices, the question arises as to what this normal price can mean. Since average prices are below the cost of production (including normal profits) then it is clear that farmers will on the average make losses over the period of the cycle. The continuance of such a position for very long is unlikely and would have to be substantiated before this ground of criticism could be removed.

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[1] There is a marked peak in farrowings in the spring. See *The Times*, March 9th, 1936.

[2] The average age at which a porker is killed is about 4 months; in the case of a baconer it is between 7 and 8 months. It should be noted, however, that since many pigs are sold as stores, the influence of the bacon market will not be as great as the figure 30 per cent suggests.

We have shown that if farmers acted according to the assumptions of the "cobweb theorem", the technical conditions are such that a cycle of about two years would arise. Since it is generally agreed that the cycle is much longer than this, it is clear that this furnishes additional grounds for rejecting this theorem. It is clear, therefore, that an explanation of the length of the cycle must be sought in delayed reactions on the part of farmers. These delayed reactions cannot, however, be of a rigid form, as we have shown in Section II where we examined this problem.

It has generally been assumed that the cycle is one of four years.[1] A study of the course of the pig-cycle in Great Britain from 1866 to 1914 shows this to be an undue simplification.[2] During this period, it is noteworthy that the length of the cycle tends to decrease and the amplitude of the fluctuations to become less severe. The Report of the Reorganisation Commission states that the fluctuations are becoming worse. This, however, is due solely to the fact that a comparison has been made between the pre-war years when the amplitude of the cycle was at its lowest and the post-war years, when the cycle was clearly affected by the abnormal character of the intervening period, while only a short period before the war has been considered. During the first 24 years of the period after 1866, there was no cycle less than five years, judging fluctuations by variations in the pig population. After the peak in 1883, eight years elapsed before the next peak population was reached, and it is interesting to observe that about this time there appears to be have been a change of trend in the pig population. From 1891 to 1914 the trend seems to have remained unchanged at the higher level and the cycle to have shortened, a cycle of three years from peak to peak being found at the end of this period. During the whole of this time, the cycle was never longer than five years. Furthermore, the amplitude of the cycle shows considerable variations during this period, but on the whole tends to be smaller. This tendency is evident in spite of the increase in amplitude during the period 1883 to 1891. If the amplitude is measured relatively to the average pig population then this tendency to lessen would be more noticeable, since the pig population was increasing towards the end of this period. The average length of cycle of 41 months, which has been generally accepted, is not therefore at all representative, for it conceals the wide variations which have taken place. The figure of 41 months, advanced by Dr. Keith Murray,[1] is an average of the four cycles between 1904 and 1928, omitting the war years. The actual cycles were 47, 37, 36 and 48 months, which indicates how misleading the figure 41 months is.

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[1] See the Report of the Reorganisation Commission.

[2] We exclude the period 1914 to 1920 because of the abnormal character of these years.

This variability in the length of the cycle and particularly the tendency to shorten, is of itself sufficient evidence of the inapplicability of the "cobweb theorem," unless it can be shown that technical factors changed in such a way as to bring this about. But this is extremely improbable, since the shortening of the cycle has been so considerable. Agricultural economists who now attempt to explain why there is a four-year cycle are having regard merely to the *average* length of recent cycles. Had this problem been investigated towards the end of the nineteenth century, the problem would have been to discover why there is a five-year cycle. This reduction in the length of the cycle between 1866 and 1914 has been completely ignored by the Re-organisation Commission. This fact has been noted by certain agricultural economists.[2] Dr. Keith Murray states that "the shortening of the period is due to the earlier maturity of pigs and increases in fecundity and fertility".[3] It seems improbable that technical changes have been so great as to cause a reduction in the cycle from six years to three; while this does not explain such increases in the cycle as have occurred. Mr. Morgan Jones suggests that the shortening of the cycle "can largely be accounted for by reason of the more rapid circulation of market information resulting in a quicker adaptation of production to prices". This may be a factor of importance, but only if the assumptions of the "cobweb theorem" are rejected.

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[1] *The Price of Porkers*, Occasional Notes, Vol. 1, No. 10, published by the Oxford Agricultural Economics Institute.

[2] See K. A. H. Murray, "The Future Development of the Pig Industry in Great Britain," *Empire Journal of Experimental Agriculture*, Vol. 1, No. 3, September, 1933, and *Factors affecting the Prices of Livestock in Great Britain*; J. Morgan Jones, "Factors affecting the Prices of Pigs in Wales," *Welsh Journal of Agriculture*, 1928.

[3] *Factors affecting the Prices of Livestock*, p. 118.

The cycle arises because of errors in forecasting on the part of farmers, and we have shown that it is impossible to find any simple and rigid relationship which will explain the course of the cycle. Technical conditions would permit a cycle of two years and since the cycle is in all cases longer than this, it follows that an explanation of the length of the cycle must lie in delayed reaction on the part of farmers. Our analysis of the reactions of farmers indicates that they hold to their views about the future price in spite of considerable changes in various factors including the actual price obtained for bacon pigs. Since farmers are slow to change their estimates of future prices and since, as we have already argued, farmers cannot be expected to estimate correctly, it follows that errors will persist over a period of some years. Apparently, however, there is a tendency for this period to shorten. One or both of two factors may be operative to bring this about. One factor is that farmers may learn from experience and thus correct errors more quickly and tend to be more accurate in their forecasting. The other is, of course, that farmers who persistently make bad forecasts will make losses and will tend to turn to alternative occupations. There will be a tendency to equilibrium and the length of the cycle will therefore depend on the point of time in this movement that one happens to be considering.[1] The time taken for this equilibrium to be reached is partly a function of the rate at which farmers learn from experience and would appear to be a problem lying rather within the province of the psychologist than that of the economist.[2]

IV

If farmers acted in the way postulated by the "cobweb theorem", the cycle of prices when it had arisen would persist even though the supply and demand curves remained unchanged. Since farmers do not act in this way, it would seem, unless farmers' reactions are of a very peculiar nature, that given these conditions there would be a tendency to equilibrium. If, however, there is a change in the supply or demand curves, this may give rise to sufficient disturbance to prevent a position of equilibrium being reached. The continuance of these fluctuations appears to be due to the existence of certain dynamic factors not taken into account by the " cobweb theorem ". If static conditions prevailed, the cycle would tend to be eliminated, although farmers continued to make errors in forecasting prices.

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[1] Of course, this tendency to equilibrium may be upset by the introduction of abnormal factors such as wars.

[2] We do not mean that more detailed work cannot be done, particularly the part played by changes in supply and demand conditions. We are referring to the general theory of the pig-cycle.

This conclusion is of extreme importance in view of the statements made by Professor Pigou in his *Economics of Stationary States*.[1] Professor Pigou is discussing the question of whether, in stationary conditions, there would be any tendency towards equilibrium if by some accident in a particular year there should be a divergence from this position. " Economists almost invariably assumed without argument that there is present something analogous to physical friction, which will make the swings backward and forward round the equilibrium position gradually dwindle to nothing. But is this assumption warranted ? Friction in macroscopic physics is a fact of experience. Whether anything analogous is at work to stop the movement of the economic pendulum cannot be decided *a priori*. It is a question of how in fact people behave. If they start one year by producing 10 per cent. above the equilibrium amount, so far as *a priori* considerations go, the cut in production which disappointment causes may equally well carry their output next year more or less than that distance below the equilibrium amount. The swings of the economic pendulum are as likely to grow in amplitude as to diminish. What actually happens can only be ascertained by a study of the facts. Far too little work has been done on this subject to warrant dogmatism. The regular recurrence of the 21-months pig cycle necessarily raises doubts."[2] It would, however, appear that a detailed study of the pig-cycle, far from supporting the possibilities which Professor Pigou is considering, confirms the view which had normally been held by economists. As we have shown, there is no evidence of a regular 21-months cycle.

Our investigation also supports the view put forward by Mr. Rosenstein-Rodan in a recent article.[3] " As long as no unique correlation between changes in actual prices and anticipations can be shown—and it is most improbable that it ever will be shown—anticipations of future prices must be considered as independent variables determining the demand in the same way as actual ('real') prices."[1] The pig industry has generally been considered as one of the few definite cases in which there was a unique correlation between actual and anticipated prices. To have shown that this is not so in pig production must make one extremely sceptical concerning the existence of such a relationship in any other field.

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[1] Chapter III, " A Thorough-going Stationary State in Relation to Stationary Conditions," pp. 11–15.

[2] P. 14.

[3] Rosenstein-Rodan, op. cit.

Our conclusion has also practical significance. We pointed out in our previous paper that if there are long-run changes in demand or supply conditions, the authorities set up under the Pigs Marketing Scheme are likely to be faced with the alternative either of ignoring long-run trends or by adjusting to them, of producing fluctuations almost certainly more violent than any that have previously occurred. We have shown that changes in demand and supply conditions tend to perpetuate the pig-cycle and we see that it is these same changes which are likely to frustrate the efforts of the authorities whose aim it is "to promote stability in a free market".[2]

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[1] P. 277.

[2] See the Report of the Reorganisation Commission, p. 63.

STATISTICAL TABLES.

Number of Pigs in Great Britain in Thousands.

Year	No.	Year	No.	Year	No.
1866	2,478	1882	2,510	1898	2,452
1867	*2,967	1883	*2,618	1899	*2,624
1868	2,309	1884	2,584	1900	2,382
1869	†1,930	1885	2,403	1901	2,180
1870	2,171	1886	†2,221	1902	†2,300
1871	2,500	1887	2,299	1903	2,687
1872	*2,772	1888	2,404	1904	*2,862
1873	2,500	1889	2,511	1905	2,425
1874	2,423	1890	2,774	1906	†2,323
1875	†2,230	1891	*2,889	1907	2,637
1876	2,294	1892	2,138	1908	*2,823
1877	*2,499	1893	†2,114	1909	2,381
1878	2,483	1894	2,390	1910	†2,350
1879	2,092	1895	*2,884	1911	*2,822
1880	†2,001	1896	2,879	1912	2,656
1881	2,048	1897	†2,342	1913	†2,234
				1914	2,634

* Indicates Peak Figures.
† Indicates Valley Figures.

The Table gives the number of pigs on June 4th of each year except 1866 when the figures relate to March.

The Duration of the Cycle.

Length measured from Peak to Peak.		Length measured from Valley to Valley	
Length in Years	No.	Length in Years	No.
3	1	3	1
4	3	4	3
5	3	5	2
6	1	6	2
7	0	7	1
8	1	8	0
Total	9	Total	9

A Note on the Supply Price of Enterprise

By K. S. Isles

In Economica for November, 1935,[1] Mr. E. F. M. Durbin argued that the conception of a short-period marginal net product of labour used in my book, *Wages Policy and the Price Level*, is logically inconsistent. But his arguments have a wider meaning than they were given in that particular connexion. If valid, they lead to the conclusion that it is impossible to formulate *any* marginal-net-productivity conception that is both useful and logically adequate. I propose here to examine this conclusion.

I

In conditions in which a marginal change in the quantity of labour would necessitate or stimulate some change in the quantities of the co-operating factors also—because their forms are not perfectly variable—the marginal demand price for labour would be equal to the value of the gross increase in output caused by employing a marginal unit, minus the costs of employing any consequent additional amounts of the other factors. The value of this marginal net product of labour can be determined only if each of the quantities on which it depends (the marginal gross increase in output, the increase in the amounts of the co-operating factors, and the supply prices of these amounts) can be determined also.

Mr. Durbin follows Dr. J. R. Hicks in holding that the *short-period* marginal net product of labour (the forms of the equipment being given) is not determinate and hence that, in relation to the short period, the above formula for competitive wage-rates does not give a definite result. But he does not use the same argument. While Dr. Hicks believes that the quantity which is indefinite is the marginal gross product,[1] Mr. Durbin believes that it is the amount which has to be deducted from this gross product on account of the co-operating factors. He is willing to concede that the conditions assumed are adequate to determine, for each marginal change in labour, the amount and supply price of the associated change in working capital. But he is not willing to concede the corresponding claim as regards either of the two elements—the work of organisation and the bearing of risk—comprising the other variable factor, enterprise. The point which I wish to establish, and which Mr. Durbin denies, is that, in the given conditions, there is only one particular wage-rate, while any given amount of labour is employed, that can yield equilibrium—i.e., that the marginal demand price for that amount of labour is quite definite.

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[1] Pp. 476–80.

II

Mr. Durbin's argument against the idea that in given conditions a marginal change in labour would be accompanied by a determinate change in the entrepreneur's work of organisation, is that the amount of this work is not necessarily increased when production expands or reduced when production contracts ; and he appears to base it on the distinction between co-ordination and supervision—on the distinction between the work of making and co-ordinating decisions and the work of seeing that they are carried out. He believes that in organising and in later sustaining a reduced labour force the entrepreneur may have to work harder rather than less hard, for " in so far as the activity of management is distinguished from that of continuous co-ordination the reduction of management labour may increase the amount of co-ordination necessary to efficiency."

Certainly the entrepreneur will work harder after reducing his labour force if he concurrently decides—as in fact he often does—to dispense with salaried supervision and to do the work himself ; so that although doing more work than before qua workman (supervision being classed with labour rather than with enterprise) he might be doing less than before qua entrepreneur. But apparently Mr. Durbin regards supervision as an element of enterprise and is intending to suggest that a decrease in the labour force, while possibly reducing the entrepreneur's work of supervision, would cause a net increase in his total work by more than correspondingly increasing the work of co-ordination "necessary to efficiency." Though such an increase may be necessary in fact, Mr. Durbin gives no reason for thinking that it is. I suspect that he is confusing two distinct things: (i) the question whether entrepreneurs do work harder in organising their businesses when they are reducing labour in time of depression, and (ii) the question whether they *need* to do so in order to maintain a given efficiency. That they do in practice work harder in these conditions can be accepted without question. And though no doubt part of the extra work which they actually do should be regarded not as enterprise but as labour, part of it may represent a genuine increase in the work of co-ordination. But even if Mr. Durbin is right in thinking not only that some increase in the work of co-ordination does take place along with a marginal decrease in labour, but also that its size is governed by a requirement that efficiency should be maintained, he is not entitled to conclude, as he does, that the increase is indeterminate. Since he makes it depend on technical conditions (which are supposed to be given) it would be indeterminate only if it did not matter whether the amount of co-ordinating work done in conjunction with any given amount of labour was large or small; we are told, however, that the amount done in conjunction with the reduced quantity of labour is that which is necessary to efficiency.

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[1] *The Theory of Wages*, pp. 20–21. Dr. Hicks asserts that we get a markedly different result according as we regard the marginal net product as being the decrease in output that would be caused by dismissing a unit of labour, or the increase in output that would be caused by taking on a unit of labour ; for, given the forms of the equipment, the dismissal of a man would virtually mean the stoppage of a machine also, while the employment of an extra man would *not* involve the employment of an extra machine. While this is true of the particular amount of labour for which the equipment was expressly designed, it is not true of larger or smaller amounts ; and the short-period equilibrium wage of even that amount of labour is not necessarily indeterminate, though in certain conditions it will be. (Cf. my *Wages Policy and the Price Level*, p. 42 n. and pp. 68–71.)

Apparently Mr. Durbin does not see where this part of his argument leads him. By supposing that an extra bit of co-ordinating work will be done if, when labour is reduced, it is necessary to efficiency, he is in effect supposing that the supply of the activity of co-ordinating is perfectly elastic; within the limits of the entrepreneur's ability the marginal cost, like that of the fixed capital within its given total, is nil. If this is indeed true the marginal net product of labour, so far as co-ordinating work is concerned, is certainly determinate, since no deduction from the marginal gross product is on its account required.

But I do not believe that the supply of the entrepreneur's co-ordinating activity is perfectly elastic. In the conditions assumed the conception of an unchanged efficiency of the undertaking, when labour is being reduced, cannot be given any definite meaning. The only satisfactory explanation of the increase in genuine entrepreneurial work is that a given fall in the demand for the commodity (the reason assumed for the reduction in labour) will cause the whole utility curve of entrepreneurial activity to undergo a definite shift. The utility of a marginal unit of this activity depends on : (i) the net increase which, in the given conditions, it would cause in income, and (ii) the utility of this increase to the entrepreneur. By reducing the amount of income that would be obtained on any given output, a fall in the demand for the commodity would raise, throughout, the marginal utility curve of entrepreneurial activity, thus causing the amount forthcoming to increase and the supply price to decrease to an extent which, given the technical conditions relating to organisation, is in each case quite definite. In so far as genuine entrepreneurial work does increase as labour is reduced, this is probably the main reason : entrepreneurs work harder not because they have to but because they elect to. In any event, however, our chief concern here has not been to find out what form the function relating marginal changes in labour and the resulting changes in entrepreneurial work takes in practice, but to show that in the conditions assumed it is determinate.

III

I pass now to Mr. Durbin's second line of attack, in which he criticises my view that there is a determinate amount to be deducted from the marginal gross product of labour on account of the second element of enterprise—i.e., risk-bearing.[1] Why, he asks, should the amount of risk employed in the firm be reduced by the discharge of labour when, by definition, the fixed capital is in the short period fixed, having, whatever the size of the output, to remain in the business in an unchanged form ? The answer is that while the dismissal of labour cannot alter the amount of risk involved in having the given amount of fixed capital congealed in the given forms, it can and does alter the amount of risk involved in working that capital. Just as there is a risk involved in turning productive power into fixed capital in the first place, so there is a risk involved in using the fixed capital to turn raw materials into finished goods. The risk that is variable in the short period is the risk entailed in incurring prime costs; the greater the amount of the prime, or variable, factors in process of transformation into finished goods, the greater, other things being equal, is the amount of risk being incurred.

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[1] Risk being used in a wide sense to include uncertainty.

Mr. Durbin virtually recognises this answer to be correct when he goes on to argue that the amount of the increase in risk caused by employing a marginal unit of labour is indeterminate. He says: "Above all, *the degree of risk involved varies with the rate of wages paid.* The risk, that is to say, varies with the rate of wages it is supposed to determine. There is no true independence and therefore no true determinateness."[1]

The real question at issue here is fundamentally a question of fact: are entrepreneurs deterred from employing an extra unit of labour, beyond what they do employ, by the risk that the resulting addition to output would fail to yield a sufficient profit, whereas if the risk were a little less or the prospect of profit were a little greater they would employ it? If this is so, then the concept of a determinate supply price of risk-bearing is established.

On the basis of this it is easy to see that the supposed logical inconsistency is unreal. It is true that the risk involved in employing an extra unit of labour varies, other things being equal, with the rate of wages paid; since, in practice, capital has to be advanced, in the form of wages, in anticipation of output. But in relation to any *given* wage-rate the amount of risk involved in employing an extra unit of labour in the given conditions, and its supply price, are determinate. Now, in the conditions assumed, where some labour is supposed to be employed, there is bound to be some wage-rate already in existence. If any entrepreneur is in such a position that, by employing an extra workman, he would increase his output by more than enough to cover the supply price of the extra amount of risk that would be involved at the existing wage (after paying for the workman at that wage and for the determinate amounts of the other factors at their supply prices), then it would pay him to employ the extra workman; it would pay him, rather than to go without the extra workman, to offer more than the current wage. In this way the wage-rate would rapidly be bid up to the point at which the diminishing amount left over from the marginal gross product, after paying the rising wage, etc., would be just enough to cover the supply price of the increasing amount of risk involved, because of the higher wage, in employing the marginal unit. Similarly, if the actual wage-rate chanced to be above this level it would very rapidly, in conditions of competition, be forced down to it. There seems no doubt, therefore, that in the conditions assumed the marginal demand price for labour, in respect of each possible total amount, is determinate. Though this rate might fail to be established at once, it, and only it, would yield short-period equilibrium. Of course it is still open to Mr. Durbin to claim that entrepreneurs do not, in reality, act in the manner in which I suppose they do. But that is a question of fact, and it is a very different thing from claiming that the argument which I built up on the basis of these suppositions is logically inconsistent.

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[1] Economica, November, 1935, p. 479.

IV

At the beginning of this note it was stated that if Mr. Durbin's criticism proved to be valid against my conception of a short-period marginal net product, it would be equally valid against any other marginal-net-productivity conception that is of practical significance, including, by implication, the conception enunciated by Marshall. This statement must now be elaborated.

In no circumstances could the criticism have any force against the marginal net product of Marshall's illustrative shepherds. This is so because, in order to make his exposition simple, Marshall provisionally assumed the difficulty away. In that example the marginal net product is really not a marginal *net* product at all, but is the same thing as the "true" marginal product. For though Marshall does not explicitly assume that the forms of the factors of production are perfectly variable, he does assume what comes to the same thing. His actual assumptions are: (i) "that an additional man would not require any further expenditure on plant or stock", and (ii) "that he would save the farmer himself just as much trouble in some ways as he gives in others; so that nothing has to be allowed for earnings of management (even when these are interpreted broadly so as to include insurance against risk, etc.) ".[1] Thus Marshall implies that if an extra man is to be employed at all, then to take him on without making any concurrent change in the amounts of the other factors would be the most profitable course. And this condition would be satisfied only if either: (i) the other factors could concurrently change their forms, or (ii) there would be no tendency for them to do so, in the particular conditions, even if they could.

Marshall hastens to add that this is an exceptional case. In doing so, however, he tells us that the marginal net product in it " plays no greater part in governing the wages of shepherds, than does that of any of the marginal shepherds on farms where they cannot be profitably employed without considerable extra outlay in other directions; as for land, buildings, implements, labour of management, etc."[2] That is, the " true " marginal product and the marginal net product are in equilibrium equal.

The fact of this equality is accepted by Dr. Hicks.[3] Nevertheless he claims that the marginal productivity theory has a deeper significance than the marginal net productivity theory. The reason which he gives is that " net productivity assumes the methods of production to be fixed; marginal productivity assumes them to be variable "; the truth being that " they nearly always are variable to some extent." But this is an unduly narrow interpretation of Marshall's argument. Marshall, as his shepherds bear witness, does not assume that the methods of production are fixed; he merely refrains from assuming that they are always perfectly variable. Indeed the truth appears to be that Marshall was attempting to give, and succeeded in giving, a formula for wages that is true in all conditions—whether methods of production are fixed or not—including those of the short period.[4] Though " true " productivity is a more fundamental

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[1] *Principles*, pp. 515–16.

[2] Ibid., p. 517 n.

[3] *The Theory of Wages*, pp. 12–14.

[4] Cf., the letter in which he objects to J. B. Clark's static state on the ground that it sets aside the notion of change: " In my view there may be no change in fact; but only because the forces tending to make change are (or for the purposes of a particular argument or illustration are supposed to be) equal and opposite . . . I could no more write one book about my Statical state, and another about my Dynamical state, than I could write one book about a yacht moving three miles an hour through the water which was running against it, and another about a yacht moving through the still water at 5 miles an hour." (*Memorials*, p. 415.)

There does not seem to be any doubt that Marshall did take the principle of perfect variability into account, even to the extent of using it, implicitly, in his illustration of the shepherds. But since he regarded it as merely the limiting case (as his still water) already covered by his conception than net productivity, Marshall's *formula*, which embodies both conceptions, has the advantage of comprehensiveness.

In my formulation of a short-period marginal net product I merely worked out the details of Marshall's general formula, as thus interpreted, in relation to the short period. Hence Mr. Durbin, in criticising my formula, is in effect criticising Marshall's also. But it can be seen that he is doing this even on the interpretation of Marshall given by Dr. Hicks. In the " true " marginal productivity formula risk-bearing presents no difficulty because, in employing an extra workman in the conditions assumed, no risk is involved. But if the methods of production are fixed (and that is Dr. Hicks's interpretation) account has to be taken of the risk involved in installing the necessary additional machinery. Other things being equal, this risk will vary with the value of the extra machinery, and this, in turn, will vary with the rate of wages paid to those by whom the machinery is made and installed. All of Mr. Durbin's arguments (including, if we are concerned with the general rate of wages, the argument that " risk varies with the rate of wages it is supposed to determine ") have, therefore, as much force against Marshall's formula as against mine. But, for the reasons given above, I find them unconvincing.

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general formula of marginal net productivity, there was no need to devise a special formula for it.

In suggesting that in Professor Pigou's hands the word " net " is otiose, Mr. D. H. Robertson (*Economic Fragments*, p. 47) seems to have overlooked the point of this. For Professor Pigou, following Marshall, quite logically uses the general formula for dealing with the exceptional case, even though the only function of the word " net " in that particular case is to show that the formula is a general one.

Book Reviews

Preface to Social Economics. By John Maurice Clark. Farrer and Rinehart, New York. 1936. xxi + 435 pp. $2·75.

To most economists of what for convenience is termed the " classical school," the very term " social economics " is apt to be suspect. For if one holds that society is nothing but the sum of its members and that the individual self-seeking after market values under mobility and free exchange is the true and sufficient way to social wealth, the adjective social seems needless and a hindrance to the understanding of economics. Now it is the primary significance of this collection of papers, contributed to American journals by one of the ablest and subtlest of American thinkers, to challenge these assumptions, and to show the inadequacy of the " static " laws to the needs for interpretation of the " dynamic " processes which constitute economic reality. To some readers the contrast between " static " and " dynamic," suggesting as it does that the classical economists regarded the *laissez faire* competition of the Victorian epoch as a permanent condition, with a norm to which all variations tended to return, will seem overdue. But, be this as it may, the older tendency of economic science to seek simplicity and exactitude by formulating laws of supply and demand of general application to current business life, without due regard to the irregularities, complexities and continual institutional changes of that life, is scarcely defensible in the face of modern factual evidence. Modern economic life shows that the wastages which occur are too large to be regarded as mere friction and are " normal " in the business cycle. Professor Clark is much concerned with the need for getting rid of the concepts " level " and " equilibrium " which are basic in the older economics, and for substituting the concept " process " as essential to a realistic study. But even more fundamental is his demand that " value " shall be given a true " social " meaning. " The ultimate problems in which humanity is interested are not those of social value in the sense of ' value in society ' as registered by market standards. Men are interested in the values of things *to* society, and they rightly demand that economics should contribute to the solution of these problems " (page 61).

Thus market values must be set to some human test, as well as individual values to some social test. But though Professor Clark recognises that these requirements bring economics into close interrelation with political, ethical, æsthetic and other processes, each with its provisionally changing standards, he does not set out any clear statement of the meaning he would attach to the broad generalisation of " human value " and to the changes which social evolution bring to it.

Perhaps the chief value of his treatment lies in his proposals for a constructive economics which should take due cognisance of psychology in the guidance, formation and determination of economic choice, on the one hand, and the institutional changes affecting such economic choice, on the other hand. Here Professor Clark discusses the relative rationality, freedom and mobility attaching to the entrepreneur, the professional expert, the investor, the labourer, the consumer, in the processes of production and distribution, to which they contribute, and the important distinction between the standardised and the unstandardised industries and processes. Competition with its largely wasteful salesmanship, combination with its restrictions of output and excessive profits, both greatly disturbing influences in a true economy, are subjected to close scrutiny. His most remarkable piece of realism consists in an " Outline of study of guidance of ' free ' economic choices," where in some twenty pages he sets out the diverse activities required by the different productive agents in the intelligent pursuit of their callings. The general effect is to show the impotence of the methods of the older " general theory " for dealing with modern economic situations.

The later chapters are largely devoted to the parts which private and public planning can reasonably be made to play in order to remedy wastes and to increase social welfare. Here we find a very serviceable distinction between the shorter business cycles and the long-range changes brought about by technological advances and accompanying alterations in control of outputs and distribution of purchasing power. " Business cycles and depressions are due to a combination of non-cyclical disturbing forces, together with the responses of a business system which causes disturbances to spread and intensify cumulatively. This quality is due to causes, partly mechanical, partly financial and partly psychological." (page 243).

In a useful analysis of " Capacity of Production and Demand " he explains the notorious failure of demand for consumer goods to keep pace with production power at the peak of a cycle, as due, in part, to over-saving of large incomes at such periods, and partly to the excessive credit expansion which stimulates undue production in the basic industries. Some measures should be taken for the control alike of price-levels, investment and credit so as to accommodate the prices of production and consumption to one another. A better handling of the resources both of capital and of labour so as to prevent the new economies of production from upsetting the social-economic system is the immediate problem for every advanced society. Professor Clark is strictly realistic and experimental in his remedies for current troubles. His contribution is directed more to a clarification of the fundamental issues than to complete solutions. The time element is of vital importance, alike in cyclical or longer periods, so that potential, as well as actual conditions of competition and capacity may be kept in view. His plea is for more systematised knowledge, psychological and financial, so as to enable the individual, the business group, the trade union, the government, to play their respective parts more efficiently in the utilisation of national and world economic resources.

J. A. Hobson.

Economists and the Public. A Study of Competition and Opinion. By W. H. Hutt. Jonathan Cape, Ltd. 1936. 378 pp. 15s.

Like Mr. Keynes' *General Theory of Employment* this is a book which everyone whose present unhappy lot it is to be called an economist should read and ponder over again and again. Seeing that in a very large measure the teachings of the two books are diametrically opposed, there is strong reason why the books should be read in close succession. In their previous publications both authors had given some indication of the points at which they would eventually arrive and, although between these points there is an immeasurable distance, they are similar in that each marks the entrance to a world characterised by economic and social prosperity. In Mr. Keynes' world this prosperity depends upon our seeing that producers are kept fully employed: appropriate activity on the part of consumers is taken for granted. In Professor Hutt's world the prosperity depends on our recognising that "The social will will be most truly realised when the greatest measure of sovereignty is vested in consumers. As consumer the individual is sovereign; as producer he is subject." While Mr. Keynes contemplates a large extension of the traditional functions of government and an encroachment on individualism, Professor Hutt sees authoritarianism in some form as the root cause of our difficulties and would set up as the main criterion of the desirable functions of the State their power to contribute to liberty. To reach Mr. Keynes' world we must discard much of the essence and implications of the teaching of the classical economists; to reach that of Professor Hutt we must get back to this teaching, accept its essence and implications, and follow where its logic will lead. Only in the intensity of their desires to assist in the establishment of a more satisfactory economic and social order is there substantial ground of agreement between the two authors.

Probably the above comparisons will have suggested—as they are meant to suggest—not only that Professor Hutt's views are widely at variance with those of Mr. Keynes, but that they are hardly in line with the preponderance of economic thought, or favourable to the policy which has been pursued, in this country even as far back as a considerable part of the nineteenth century. In his earlier book on collective bargaining, Professor Hutt revealed that there was very little in the reasoning which had ever been adduced in support of collective bargaining which he could accept as completely valid. Presumably, according to his present book, he would insist that the weakness of this reasoning was that it was influenced by " custom-thought "—consequent upon the intellectual inertia of the community—and " power-thought "—the conscious or unconscious falsification of thought as a means to the attainment or maintenance of power. He now indicts economists for the same weakness—due to the same influence—over a wider range of their thought and to this weakness attributes the impotence of the " orthodox " economist to influence opinion. Only when " rational-thought "—the result of a successful contest against the accumulated force of custom-thought and power-thought—becomes dominant has Professor Hutt much hope that this impotence will disappear.

Somewhat strangely, as it appears, Professor Hutt is confident that he already knows what the outcome of the process of rational-thought would be and also what is the policy to which it would lead. It would be a policy under which (with certain qualifications) unfettered competition would be maintained, though it should be added that the restraints involved in this maintenance ultimately appear to go far beyond anything which has yet been seriously contemplated in this country. However, Professor Hutt anticipates this consideration by the contention that restraints are not resented when they are felt to be inevitable, impartial, or impersonal. It is, in fact, just because under existing policy restraints are allowed which have not these characteristics that he so strongly disapproves of the policy. In the economic field his policy of " consumers' sovereignty " may be regarded as a re-statement of the policy laid down in the famous dictum of Adam Smith amplified to the extent of denying the social justification of voluntary association in that field.

Admittedly all this teaching sounds very extraordinary at the present day but no serious student can afford to ignore it on that account. Rather he ought to give it the more earnest consideration and if, like the present reviewer, he finds that it raises more doubts than it resolves, it will most certainly serve the vitally important function of inducing him to examine the more or less unconscious assumptions which necessarily lie at the basis of his thought. Maybe some of the doubts that will be raised will be of the following character. The first doubt depends upon a recognition that Professor Hutt's treatment of his subject is highly intellectual—as it has a right to be ; it seems almost like an exercise in logic. In a world in which logic were the dominant impulse to action the treatment might well be regarded as more acceptable than in the world as we know it where logic is perhaps so strongly tinged with emotion as to make the latter and not the former the dominant impulse. Maybe the custom-thought and the power-thought on which Professor Hutt insists are to be thus explained, but the explanation does not get rid of them. Are they not, with logic, part of the data which the social scientist must take into account when thinking of practical policy ? Again, on economic and social questions, what is rational-thought ? What are its criteria and what is the policy to which it should lead ? Professor Hutt holds that it is a policy which would give scope for competition freely to operate (Incidentally, one would like to know in more concrete detail what Professor Hutt means by competition). But suppose someone insists that a policy which gives this scope is not desirable, are we justified in saying that his thought is non-rational ? In this respect rational-thought would seem to have relation to the end in view. This end may not be one which Professor Hutt approves, but can the thought be regarded as non-rational because of that ? While the present writer would cordially agree that the primary work of the economist should be performed in his study and in his class-room, and would insist that nothing ought to emerge from them either in speech or writing which does not bear the mark of a calm and disinterested search for truth, he is very dubious of the time ever arriving when on questions of economic and social policy there will be complete unanimity of view among men who, as far as in them lies, are moved by rational-thought.

This raises another doubt concerning Professor Hutt's treatment of his subject. It is a hard saying that the long line of distinguished thinkers who, for more than half-a-century have given their support to the establishment of systematic machinery for industrial negotiation —usually in the belief that it would assist towards the attainment of results such as the free-working of competition would achieve—have been so influenced by non-rational thought as to be mistaken. Professor Hutt's strictures cannot be examined here but perhaps it may be permitted to one who, during the last seventeen years, has had a large and continuous practical acquaintance with such machinery to say that he has undertaken this work in the confident belief that it has rational theoretical justification and that his experience has confirmed that belief. May he make the kindly suggestion to Professor Hutt that he should do likewise and see whether or not it will cause any modification of his present views ?

Much that one would like to say on the teaching of this thought-provoking book cannot be contained in a brief review but in conclusion one other word must be said. Very few who read the book will, one imagines, find it possible to resist its central argument that much which is at present wrong with the world could be righted were more rational-thought to prevail, and were the forces of competition allowed to operate more freely. No liberal-minded person can possibly listen to what Professor Hutt has to say without a very large measure of sympathy, and none can fail to see the grave political difficulties that have to be faced when liberal ways of reform are departed from. Sometimes one is inclined to think that much of the present trend of economic and social thought and policy will not permanently persist. If it does not thus persist then Professor Hutt may look to the future for his reward in a far wider acquiescence in his teaching than it seems likely that it will receive in the present. Meanwhile, he is to be sincerely complimented on his intellectual achievement and on his courage in writing his book.

G. W. Daniels.

World Population: Past Growth and Present Trends. By A. M. Carr-Saunders. Clarendon Press. 1936. xv + 336 pp. 12s. 6d.

Population Movements. By Robert R. Kuczynski. Clarendon Press. 1936. 121 pp. 5s. net.

The Struggle for Population. By D. V. Glass. Clarendon Press. 1936. 148 pp. 7s. 6d. net.

In making a survey of *World Population: Past Growth and Present Trends*, Professor Carr-Saunders has rendered a signal service. He has collected in a single volume of moderate size—well-arranged and clearly and forcibly written—the essential facts about the most important problem of our time. He has thereby, one may hope, done something to hasten the coming re-orientation of the social sciences.

For half a generation, since the Great War ended, the attention of economists, and of statesmen in the economic field, has been concentrated on problems of money, exchange and international trade. These problems have not been solved; it would be truer to say that, in practice, we have abandoned hope of solutions as complete as once seemed possible. But solved or shelved, they can no longer hold the centre of the stage. The problem of the future is not money but population. The centre of the social sciences in the next generation seems destined to be found not in economics as we have known economics since the war, not in finance or business, but in social biology and in study of the varying conditions under which the mass of mankind are born and live.

This re-orientation is called for by the second of the two great events in human history which are the twin themes, alike of Professor Carr-Saunders' survey and of Dr. Kuczynski's smaller but no less admirable study of *Population Movements*.

The first of these two events is the decline of mortality among European races which began, so far as we can fix the date, about 250 years ago. This, rather than a rise of natality, has caused the immense expansion of these races. "The total number of persons of European stock in the world" according to Dr. Kuczynski "is now about 720 millions as against about 155 millions in 1770". The decline of mortality and increase of population began before 1770, but how long before is not certain. But it is clear that "prior to the eighteenth century there cannot have been a definite population trend". As Dr. Kuczynski puts it, "if the white population had increased since 1300 as it has increased since 1770 it would now number something like 40,000 millions", that is to say about twenty times the total population of all races in the world. This first event—decline of mortality—represents in the main the achievements of preventive and curative medicine. It has spread, in varying degree, from the European races to the other races.

The second event is the decline of natality that began in nearly all countries of European stock—France, Ireland and the United States are exceptions differing among themselves—between fifty and sixty years ago. Professor Carr-Saunders, adopting the methods invented by Dr. Kuczynski, presents in a comprehensive table the net reproduction rates of various countries, so far as they are known from 1871 to 1934. Throughout the north and west of Europe, in Australia and in New Zealand the rate is below unity, that is to say, the population is not replacing itself, and, if fertility and mortality remain unchanged, will eventually decline, and disappear; for the United States, Professor Carr-Saunders' table shows a net reproduction rate, in 1930, still just above unity, but Dr. Kuczynski includes this country with those whose population no longer replaces itself.

The decline of mortality 50 years ago, like the decline of mortality perhaps 200 years before then, is the result of human inventiveness—of birth control either become much easier or through spread of knowledge become general instead of rare. In North West Europe the decline has already gone so far as to make the problem of maintenance of an adequate population not merely important but urgent.

This does not mean that there is likely in the very near future to be any marked decline in population. Until the changes of age constitution involved in the transition from high to low birth rates have worked themselves out, and this is likely to take a generation at least in England, the effect of a net reproduction rate of 0.75 such as that now reached in England and Wales will hardly begin to be felt. But, thereafter, if there is no change, population will decline by a quarter in each generation.

The urgency of the matter lies, not in the prospect of an immediate decline of population, but in the length of time that will be required to correct the operation of present tendencies. All this is clearly stated by Professor Carr-Saunders. Only on two minor points does his exposition seem open to criticism.

First, it may be doubted whether his explanation of how the decline of natality began is adequate. He dismisses, without explicit reference, the suggestion made by myself in an article in ECONOMICA in 1925 that the sudden and simultaneous spread of birth control in nearly all countries peopled by European stock 50 years ago must be ascribed to something like an "underground invention" of more acceptable means of control. He may be right in this, but if so it makes this revolutionary change in history an unexampled result of disinterested propaganda.

Second, Professor Carr-Saunders is perhaps more definite than our knowledge warrants, in asserting the difficulties of reversing a decline of natality. Once the reproduction rate has fallen below replacement rate, " the restoration of a replacement rate proves to be an exceedingly difficult and obstinate problem ". To this the reply can fairly be made that there has not been enough experience yet to prove anything about this.

There is, of course, no hope of returning to a replacement rate in Western Europe, either by a further decline of mortality or by an increase of marriage. The decline of mortality needed for this purpose is that of women between 15 and 50 : as all girl babies born in England already live on an average through 30 out of these 35 years, the room for further improvement is small.

The possibility of raising natality by making marriages more general even than at present is slightly greater, but also quite inadequate to bring the reproduction rate up to replacement level. Professor Carr-Saunders rightly makes it clear that the only means of preventing the ultimate extinction of the British population is an increase of fertility under conditions in which every child will be a wanted child. What are the possibilities and methods of changing men's minds in this direction ? To this issue the third book here under review is addressed. *The Struggle for Population* by Mr. D. V. Glass is a valuable and up-to-date account of the methods adopted in France, Belgium, Germany and Italy to increase population. The general effect of this study is to suggest that the methods hitherto adopted have had relatively little success. But this, though valuable so far as it goes, proves little as to the future.

The time has been too short for judgment. And as Professor Carr-Saunders points out, the economic inducements offered and the steps taken to remove the obstacles to fertility in most countries hitherto have been ludicrously inadequate. They throw no light upon what might be accomplished under social and economic arrangements which made bringing the next generation into the world not merely a little less difficult, but so far as was humanly possible a safe and attractive occupation.

It is true, as Professor Carr-Saunders says, that " the parental instinct is now satisfied when the average size of the family is not sufficient for the purpose of replacement ". But this statement has to be taken with two comments. First, the word " now " must be emphasised as meaning under existing social arrangements. Second, the parental instinct is almost certainly not a force of equal intensity in all individuals. It may well happen that in future population will be increasingly and adequately recruited from those particular individuals in whom the instinct is strong or the feelings restricting fertility are particularly weak.

These comments, while representing a criticism of some of the phrases used by Professor Carr-Saunders, are in substance a reinforcement of his main argument. If a restoration of fertility to replacement level were a hopeless affair, it would not be worth attempting, but the more the problem can be made to appear capable, rather than incapable, of solution, the more urgent it is to apply the solution.

It is inevitable that this review should concern itself chiefly with the central fact about population to-day—the fall of fertility below replacement level in nearly all the European races. But the books under review are not confined to that theme.

Mr. D. V. Glass gives a well-documented, though brief account of all existing schemes of family allowances, including that in force at the London School of Economics and Political Science.

Dr. Kuczynski's opening chapter is an interesting account of the simultaneous peopling of America with blacks and with whites—about 15,000,000 negroes and about 40,000,000 Europeans coming as permanent immigrants since 1492. And the " strange fact " which he mentions in another chapter, that in England the mean expectation of life of those who reach 60 is little more than it was in 1845—is 15.4 years as against 13.9—will certainly be strange to many readers : " notwithstanding all progress in hygiene and medicine, mankind nowadays is just as unable to extend the life of old people as a century ago ".

Professor Carr-Saunders does not deal with all the problems of population. He says very little about differential fertility within each country. And he does not discuss such matters as the effect of varying densities of population upon industrial productivity or the standard of life. The economics of population generally he treats as outside his range. But his survey of the course of population is wide and full. He attaches well-deserved importance to migration. Of special interest is his account of Japan and India. As to Japan, he comes to the important conclusion that birth-control is at work. Of India he gives a gloomy picture. In India the population lives not at, but near to, the margin of subsistence, with a birth rate showing no definite trend and a death rate fluctuating with harvests and epidemics. The country, or a large part of it, is over-populated and three of the most urgently needed social reforms—better health services, abolition of child marriage and re-marriage of widows, would all intensify this evil. " There could scarcely be a more tragic situation ".

Here are three books, all deserving the closest attention from all students of society.

W. H. Beveridge

The Clash of Progress and Security. By Allan G. B. Fisher. Macmillan and Co., Limited. 1935. xiii + 234 pp. 8s. 6d.

In reviewing this book I feel that it belongs to that type where a rather vague and insignificant title may hide from a wider public the great importance of the contents. My main—and since there is nothing serious to criticise, almost my only—task is to indicate the true significance of what Professor Fisher is driving at in a most successful manner. His point may be summarised as follows: (1) A progressive economy, *i.e.*, an economy with an increasing degree of want satisfaction, is characterised by a continuous change in the structure of production brought about by changes in taste and by changes in productive technique. (2) This continuous change in the structure of production, salutary as it is for the community as a whole, is a process more or less painful for the corresponding producers and encounters, therefore, a more or less violent resistance on their part. Consequently, there is in our economic system (or better, in any highly differentiated economic system) an inherent disharmony between the common interest and the sectional interests of the different producers. This disharmony—" the bacillus of the disease of civilisation " as it is called by Wicksteed in one of the most impressive chapters of his *Common Sense of Political Economy*—has always been the starting point of those economists of the Liberal school who, curiously enough, are being sneered at as " Harmonists " because they are tactless enough to suggest unhampered competition as the only effective remedy against the disease. (3) If changes in demand and in the technique of production are not followed by corresponding changes in the structure of production, grave disturbances in the form of unemployment and " excess capacity " are bound to occur. Economic progress has, then, been frustrated and, what is more, has become the cause of poverty, misery, disappointment and discomfort. " Progress and Poverty," " poverty amidst plenty " and other slogans become popular and give to the engineers as the representatives of technological progress the well-known air of superiority over the poor economists as alleged representatives of sluggish capitalism. (4) For various reasons, changes in taste and in technique are to-day greater than ever, but unfortunately the resistance to the necessary transfers of productive agents has also stiffened to a degree unwitnessed before. This is the situation which the world of to-day has to face and against which a remedy has to be found if we want to get out of the present impasse and to avoid the most threatening dangers. Professor Fisher, accordingly, concludes his book with a chapter in which certain practical proposals are discussed and with another chapter showing the fate which awaits capitalism if no solution is being found.

I have given more or less my own version of the main trend of Professor Fisher's book, but I believe it gives a fairly true idea of his most important points. That he has hit on one of the gravest defects in the construction of the present society will be a matter of universal agreement, but I also like to think that everyone who reads his book will agree that the job has been done with a forcefulness, a competence and an intellectual courage which can hardly be surpassed.

It is really a book as lucid, as cool and as refreshing as a mountain lake, and it is to be hoped that it will arouse the vivid echo which both the book and the case defended by it deserve.

In its details, the book opens vast horizons and almost boundless fields of discussion. It is perhaps a mild criticism to say that some of the problems raised are indeed so complex that it seems impossible to do them real justice in a few lines or even pages. The problem of agriculture (where the relatively high degree of demand elasticity for the more refined products such as butter, meat, etc., has perhaps not been duly reckoned with) and the problem of the market of the academic professions may be mentioned in this respect. More serious, perhaps, is the question whether the author has not in some respects overstrained his argument. The rigidity of our economic system, brought about by the muzzling of competition, is really bad enough, but is it quite just to treat the resistance to change of vested interests on the same footing with the timid resistance of newcomers ? While even the former is possibly subject to cyclical fluctuations, this is certainly true of the latter. Given the normal atmosphere and the normal functioning of the financial arrangements of our economic system, there still seems to be small reason to suspect that " capitalism " has lost anything of its restless search for new lines of investment. In this respect, it seems dangerous to generalise from the experiences of a depression which surely has been caused by circumstances that have very little to do with entrepreneurial sluggishness as such. For the same reason, it is very difficult to follow the author in his suggestion to see in the contrast between the necessity and the actual slowness of economic change one of the major causes of cyclical fluctuations, though it must be conceded that it might be a contributing and aggravating factor of the first magnitude, and that there are problems here which have been somewhat neglected by the cycle theorists. In this respect, it is safe to assume that in the future cycle theorists will give more attention than heretofore to " horizontal " misdirections, *i.e.*, to misdirections *within* the sphere of the production of consumption goods or of capital goods.

The last pages are very stimulating but also especially provocative of critical comment. I am altogether in sympathy with Professor Fisher's feeling that it is of no avail to wring one's hands in despair at the deserted altars of Liberalism without thoroughly exploring the reasons of the desertion, and I also think that we have to be grateful to him for having helped us a good deal in this task. It is certainly not true, however, that orthodox economics has been blind to the clash of social and sectional interests which Professor Fisher has exposed so forcefully. It is not for nothing that orthodox economics has always so much insisted on the necessity of unhampered and undistorted competition for breaking down sectional resistance. Why this fight in favour of competition has become less and less successful is another story, on which quite recently Professor Hutt has thrown much new light in his excellent book on ***Economists and the Public***. Liberalism, indeed, if it has been worthy of this name, has always been a fight against the "capitalists" in order to guarantee the functioning of the economic system for the sake of the communal interest. In this, it has always been a matter of independent and rather lonely thinkers whom it is grossly unfair to lump together with "business interests." Professor Fisher belongs to them *malgré lui*.

A last word must be said about Professor Fisher's "Challenge to Capitalism" which occupies the last pages of his book. In contrasting the case of capitalism in its present deplorable state with the case of socialism, his attitude becomes rather ambiguous. It goes without saying that everybody who by his own behaviour impairs the working of capitalism is strengthening the case for socialism, and, as a tactical move, something might be said for sharpening the "capitalists'" sense of responsibility by threats to abandon the case of capitalism. But can the author seriously maintain that just in this respect socialism offers any better prospects than capitalism, even than capitalism at its worst? Quite apart from the fact that there is just as strong resistance to change under socialism as under capitalism (as everybody with experience of government departments will agree), it is all too probable that under socialism the clash between the necessity of and the reluctance to change will be avoided by removing the former. That under capitalism there *is* that tremendous technological progress which is the main starting point of Professor Fisher's book, while under socialism both reasoning and experience make its absence highly probable,[1] shows the extent to which the opinions of the author in this respect seem to be untenable. From the author's own point of view, every flirtation with socialism, though pardonable as an outbreak of exasperation, is extremely dangerous, while now as ever the only hope to make economic progress a boon instead of a curse lies in rearranging the institutional framework of our system in such a way as to break down resistance to change by competition—a statement which has to comprise all modifications and concessions which a realistic conception of the problem imposes.

WILHELM RÖPKE.

Der Internationale Handel. By Gottfried Haberler. J. Springer, Vienna. 1933. xi + 298 pp. Rm. 19.60.

The Theory of International Trade. By Gottfried Haberler. Translated by Alfred Stonier and Frederic Benham. William Hodge. 1936. xv + 408 pp. 21s.

The publication of Professor Haberler's treatise on the theory of international trade and the rapid appearance of an excellent translation by Messrs. Benham and Stonier are events which should be welcomed in many quarters. They should be welcomed by students of the subject for their various contributions to the advancement of this difficult subject and they should be welcomed, too, by teachers as a most lucid systematisation of a body of knowledge often only accessible hitherto in scattered articles in the periodicals or in the oral tradition of a small number of special institutions. In the past, as any who have had experience of large public examinations know, the lack of accessible textbook treatment has made it impossible to require, except in the most advanced papers, anything but the most rudimentary knowledge of many quite essential concepts. Many boards of examiners would regard, for instance, a question involving any knowledge of the distinction between the factorial and the commodity terms of trade as advanced theory, too difficult for any but specialist students. Henceforward it should be possible to insist on much more respectable standards.

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[1] As for the aptitude of a socialist state to technological progress, it is of interest to consult a book written by an Italian engineer on Soviet technique (G. Ciocca, *Giudizio sul bolscevismo*, Milan, 1933). The author gives very interesting examples of the obsoleteness of machinery which, when it was installed a few years before, represented the last word in "capitalist" engineering. Under the conditions of a socialist régime it can hardly be otherwise.

In a sense the title of the book is a misnomer. Professor Haberler covers not only the general theory of international trade: he provides also sections occupying nearly half the book, in which trade policy in its various aspects is described and analysed. Even in the theoretical sections he keeps his feet firmly on the ground, continually justifying his assumptions by reference to practice and illustrating his conclusions by historical examples. For specialists, no doubt the chief interest of the book will be in its exposition of theory. But teachers at least will find those sections which discuss the technique of trade policy almost equally valuable. There is no other place in the literature where, side by side with the exposition of principle, there is to be found so compact and systematic treatment of such matters as the technique of tariff making, the form of commercial treaties, the pros and cons of the debate regarding the Most Favoured Nation Clause and similar matters. The old-fashioned title, Principles of International Trade, would in many respects have been a better description of the contents. It might, moreover, have been less of a deterrent for those who think that theory has nothing to do with practice.

Much of the difficulty of teaching Economics arises from the close interdependence of the different parts of its subject matter. It is always a problem to know at what point to cut into the circle. In arranging the theoretical part of his work, Professor Haberler makes some break with tradition by treating the monetary problems of international trade first and dealing with the pure theory only at a later stage. This obviously has considerable pedagogical advantages. It starts from what is familiar, and it is not until the student has been surreptitiously drilled in a good deal of abstract thought, that he is introduced to the high abstractions of comparative costs, reciprocal demand and so on. None the less, I am inclined to think that it lacks something of the logical force of the usual order and, although Professor

Haberler succeeds in expounding the outlines of the general theory of transfer probably more clearly than any preceding writer, I think it is arguable that, if he had had his pure theory behind him at this point, his exposition might have been even more successful.

It is certainly in the section on pure theory that Professor Haberler makes his most important contribution. He starts in traditional fashion by the assumption of simple (*i.e.*, single-factor) production in the different areas. The theory of comparative costs and reciprocal demand are expounded in a setting of labour cost reckoning. But, having paid this tribute to tradition, Professor Haberler then proceeds to modernise his apparatus. The theory of comparative costs is re-expounded in terms of sacrificed opportunities. The Ricardian theory is shown to be merely a special case of the general proposition often known as Wieser's Law; and the diagrammatic instruments of Fisher and Pareto are invoked to give precision to the analysis. Professor Haberler's constructions here have already become so much part and parcel of the contemporary apparatus of analysis that it is possible that some of his readers may fail to realise the considerable originality of this part of his work. But few will fail to enjoy the elegance of the treatment with which it is thus shown that international trade theory is merely a specially well worked out and more than usually instructive series of corollaries of the broad principles of the general theory of production.

Professor Haberler's treatment of the main propositions of the traditional treatment of the subject is so much more exhaustive than anything that is to be found elsewhere, that it may seem carping to call attention to small gaps. Nevertheless, I believe that there are certain additions by which future editions could be made even more helpful. There is missing a systematic analysis of the important case of two countries competing in a third. Few works seem to give this the prominence it deserves. Yet it is not an intellectual plaything but an indispensable clue to much that is important in contemporary history: and its absence suggests a belief in a degree of economic harmony which it is doubtful whether any economist really entertains. I could wish, too, that Professor Haberler had paused a little longer in his elucidations of the phenomena of both real and monetary changes in the terms of trade. A section here devoted to the problem of the symmetry of import and export duties might greatly elucidate matters which are taken for granted in much of the literature of the subject, but nowhere, to my knowledge, very systematically discussed.

Professor Haberler's own criticism of his book in the preface to the English edition is that it does not contain sufficient analysis either of imperfect competition or of the international aspects of short-run oscillations. Both these criticisms are perhaps just. But they are criticisms not so much of the book as of the present state of theory. It is not clear how much we really do not know as regards imperfect competition in international markets. My impression is that most of the knowledge does exist, scattered about in works on particular problems, and that the main business here is to give it coherent shape. But as regard short-run oscillations there is no doubt much more to do. The theory of the exchanges especially has been developed in the shadow of the great inflations of the twenties; and, although in the context for which it was intended—the analysis of large fluctuations when the system is fully extended—it is a most powerful and sufficient instrument, recent experience suggests that it could be very profitably extended to take account of the effect of smaller variations when the system is partially collapsed. The success with which Professor Haberler has succeeded in systematising and advancing existing knowledge on these subjects leads us to hope that at some future date he himself will find time to fill in the gaps he has indicated.

On page 136 of the English translation, in the formulæ of the fourth line from the bottom, certain symbols have got badly misplaced. Having regard to the importance of these formulæ for the understanding of this part of the argument, the possibility of inserting an erratum slip well deserves consideration.

LIONEL ROBBINS.

Prices in the Trade Cycle. By GERHARD TINTNER. Vienna: Julius Springer. 1935. xii + 203 pp. Rm. 23.80.

In this most welcome volume Dr. Tintner gives the results of a statistical study of pre-war prices, the data being drawn mainly from England (1845–1914) and Germany (1879–1914) and used in conjunction with Professor Mills' material for U.S.A. (1890–1914). A general survey is thus obtained of the changes in the price structure of three important countries in the years before the war.

To assess the value of this work, a sharp distinction must be drawn between the statistical technique used and the interpretation of the results from the point of view of the Trade Cycle. The first object of the statistical analysis of the data is the isolation of the cyclical fluctuations by the elimination of trend, seasonal and casual variations in each price series. The method of variate differences and moving averages is here employed, as explained by Dr. Tintner at some length. A number of interesting side-lights are thrown on the statistical problem and it is useful to be given, for example, a formula correcting for the reduction of cyclical amplitude resulting from smoothing by moving averages (p. 85). The second object on the statistical side is the devising of a series of statistical measures to serve in the comparison of different price cycles, of two price cycles in one country or of the same price cycle in two countries. Dr. Tintner has defined about a dozen such measures, well adapted for his later interpretations. Some of these (the mean length and amplitude of a price cycle and so on) are obvious measures; others, such as the fixing of the sequence of entry of different prices into one cycle, are more ingenious.

Without an examination of the detailed work involved, it is not possible to judge the success of these statistical methods as applied to the English and German data. The practical difficulties of dealing with such imperfect data are clearly very great and the investigator must often fall back upon his own common-sense judgment. Even in a simple estimate such as the length of a price cycle from one high point to the next, there is considerable margin of error. From Dr. Tintner's general account, it would appear that we can take the statistical analysis as most carefully accomplished.

The basis of Dr. Tintner's comparative study is an international Trade Cycle system given by Thorp and Mitchell, *Business Annals* (1926). This system consists of dated high and low points of successive cycles 1845–1914 and it is fixed quite apart from the price data as here used. It is, in fact, a system to test, and be tested by, the new price data. As a result of the comparison between the various price cycles and the fixed system, it is fully established that the U.S.A. showed a pre-war Trade Cycle, averaging under four years in length, in which about 80 per cent. of the separate U.S.A. prices regularly took part. The situation in industrial Europe (as represented by England and Germany) is seen to be very different. The outstanding Trade Cycles here were of 7–8 years average length and coincided with only half the U.S.A. cycles. As many as 85–90 per cent. of the individual prices examined took part in these " second rhythm cycles " (to use Spiethoff's term). There were also secondary cycles, corresponding to the remaining U.S.A. cycles, in which a smaller number (about 55 per cent.) of the prices took part. So, for example, metals and agricultural products show only the longer price cycle whereas textile prices and the interest rate conform to the shorter U.S.A. cycles. The complex nature of the European Trade Cycle is most important : the long cycle over the whole field of prices and the extra cycles in step with the U.S.A. over a more limited field. The latter tend to be overlooked.

Dr. Tintner then proceeds to a full analysis of the contribution of various individual prices to the general Trade Cycle. This proceeds, not only in terms of the varying lengths and amplitudes of each price's cycle, but also with the aid of more interesting and less obvious concepts such as the sequence of entry of prices, the relative length of boom and slump and the relative high and low amplitudes of individual prices. The prices of the various countries are classified and grouped in some most illuminating ways and the results, though incomplete and tentative, are of great interest. The contrast between the long and strong cycles of the metal product prices and the shorter and weaker textile price cycles is well made. Special emphasis is laid upon the importance of the interest rate and the prices of semi-finished goods in the Trade Cycle, and upon the weak cycles displayed notably by monopoly prices and the prices of consumables. More than one arresting conclusion is reached. Dr. Tintner maintains, for example, that luxury consumables have price cycles of *small* amplitude and that "capital goods" (particularly semi-finished goods) appear *late* in the boom, and later still in the slump, as compared with "labour goods."

At the risk of appearing ungrateful, it can be added that the exposition is often not clear and the reader is sometimes left to disentangle for himself the precise meaning of terms and classifications used. This is, of course, largely due to the fact that Dr. Tintner is writing in a language not his own. But there is also much avoidable repetition in the later sections of the comparative study. On the other hand, the book is lavishly produced and two envelopes attached to the end cover contain a series of beautiful graphs of individual price cycles, each reproduced on a separate sheet of transparent material.

R. G. D. Allen.

Wages, Cost of Living and National Income in Sweden, 1860–1930. By the Staff of the Institute for Social Sciences, University of Stockholm.

Vol. II. *Wages in Sweden*, 1860–1930. By Gösta Bagge, Eric Lundberg and Ingvar Svennilson. Part II, "Government and Municipal Services, Agriculture and Forestry; The General Movement of Wages in Sweden, 1860–1930." London: P. S. King and Son, Ltd. 1935. xix + 393 pp.

This book is a further instalment of the monumental series upon which the Institute of Social Sciences at the University of Stockholm has been engaged. Vol. I, on the "Cost of Living in Sweden, 1860–1930," and Vol. II, Part I, "Wages in Manufacture and Mining," were reviewed by Professor Bowley in the issue of this Journal of August, 1934 (New Series, No. 3). We here have the second part of Vol. II, thus completing the survey of wages, and it is stated therein that Vol. III, "The National Income of Sweden, 1861–1930," is in the press. When the whole series is thus available, a most ambitious project will have been carried through, and one which will be of interest not only to Sweden, but to economists all over the world.

As has been said, Part I dealt with wages in Manufacture and Mining. Part II now contains studies of wages in Government and Municipal Services, and in Agriculture and Forestry, and concludes with a survey of the General Movement of Wages. For Government and Municipal Services the same technique was used as for Manufacture and Mining, and as this technique was summarised in Professor Bowley's review, repetition seems unnecessary. Notes on the different towns correspond in miniature to the detailed studies of separate establishments in Part I. From 1860–90 the available data is too scanty for any reliable series, but from 1890 onwards statistics of hourly and annual earnings of skilled and unskilled workers in a sufficient number of towns have been collected. Annual real earnings of unskilled workers are shown to have risen by 80 per cent. between the periods 1890–93 and 1910–13; and by rather more than another 80 per cent. between 1910–13 and 1927–30: hourly earnings rose by the same amount as annual earnings up to the war, but the rise since 1914 is over 120 per cent. owing to the shortening of the working week.

Turning to railway wages, no data is available for the private railways, and the enquiry is therefore confined to the State railways. This is a pity, first because the private railways owned about double the mileage of the State railways during most of the period, and secondly because comparisons of wages between the two would have been interesting. The technique employed is different. Up to 1906 the best data is contained in the Staff Rolls which give the names etc., of all employees and their regular wages each year. The investigation is confined to the Stockholm district owing to the amount of work involved. Complications are encountered owing to the practices of giving free housing or rent allowance and mileage bonuses to drivers and firemen, but these seem to have been fairly satisfactorily surmounted. Four categories of workers are selected as representative, and adequate statistics are assembled, but no attempt is made to estimate the averages for all employed, and thus no index representing the general movement of railway wages is obtained. Since 1906 complete official statistics are available. Without wishing to appear dissatisfied when so much has been done, the hope may perhaps be expressed that yet more work will be done on this chapter before the next edition.

The section on agricultural wages is of course the most important of all from the purely quantitative point of view, since in 1870 over 70 per cent of the total population was employed in agriculture and subsidiary occupations, and even in 1930 roughly 40 per cent. The usual difficulties of dealing with agricultural wages are intensified by the different and numerous grades of agricultural workers in Sweden, each with its peculiar and widely differing status, and by the prevalence of wages in kind. On the other hand, an unusually good source of information is available owing to the requirements of the taxation system (the Markegång Scales), and this provides, so to speak, a solid core of data, though limited in its scope and utility, round which can be built up a larger edifice with the aid of information from the annual statistics of the Agricultural Societies and from investigations by the Institute itself. Great pains have clearly been taken to secure the best possible results compatible with due scholarly caution, and if the results achieved are not entirely satisfying, they are a great deal better than might have been expected in view of the difficulties and complexities involved.

The section on the General Movement of Wages, 1860-1930, opens with a chapter on the Swedish Labour Market, which, especially from the foreign student's point of view, is an admirable piece of work. Perhaps the authors will consider whether in the next edition the whole, or large sections of it, would not be better placed before, rather than after, the wage enquiries. In the next chapter the series for wages in manufacture and mining are compared with the trends of production, prices, rates of interest, investment in capital equipment, and so on. This leads on to a study of " Industrial Wages and the Secular Changes in the Labour Market," and to a brief survey of variations in wages during business cycles. These chapters contain much of general interest for the students of wage theory, but one has a feeling that they will be still more instructive when the volume on the National Income becomes available, and that a certain amount of re-writing and re-arrangement in the light of that volume may ultimately yield still more productive results. Finally the trend of industrial wages is compared with the trend of agricultural wages (a very difficult business owing to the different character of the available series) and with wages in the public services, while the last, but not the least, interesting chapter is concerned with local trends in wages.

Many students in other countries will envy Sweden the possession of such a series as the Institute is providing. To say that this volume on wages represents the last word which can be said, would be a compliment to the authors as fulsome as they must hope it is false. But the Institute has broken the back of a stupendous piece of work: to improve and to supplement will not perhaps be easy, but it will be relatively easy.

J. W. F. Rowe.

The International Banks. By A. S. J. Baster. P. S. King and Son, Ltd. 1935. vii + 269 pp. 12s. 6d.

Mr. Baster's previous book, *The Imperial Banks*, has taken its rightful place among the valuable descriptive studies of British banking which have appeared in recent years, and the present book is intended as a supplement to the earlier one.

The international banks have always been considered a distinct section of the money market, both on account of the work they do and on account of the difficulties they have to face. Their main function is to transfer capital from one country to another, and their difficulties arise from the very fact of operating abroad. When the laws of a country permit the establishment of foreign banks in its territory (and there is by no means universal freedom in this respect) there remain certain operating difficulties. A staff has to be trained to the different banking conditions overseas; connections have to be made in new territory; provision has to be made against exchange fluctuations, for whilst current receipts and disbursements can be adjusted to avoid or mitigate the consequences of such fluctuations, long term investments cannot be covered so easily.

The story of the extension of banking outside the boundaries of the capitalist countries of Western Europe is partly the story of private enterprise seeking new opportunities for the profitable use of capital, and partly the story of political aspiration. This mingling of economic and political motives is clearly reflected in the use that Mr. Baster has made of Foreign Office papers. In the case of the Bank of Egypt, for example, the Consul-General at Alexandria thought the foundation very desirable and deserving of encouragement "upon political as well as commercial grounds." The position was similar in Turkey proper; as Mr. Baster explains, "Owing to the political importance of Turkey's ultimate destiny, the activities of these banks [the Ottoman Bank, the National Bank of Turkey, and the Deutsche Bank] were turned into a peculiar mixture of finance and diplomacy."

In South America conditions differed in some respects. British banks, the first in the field, relied mainly on existing commercial connections, but were hampered by the instability of the local governments. Indigenous banks were frequently set up and given preference. Later, when both economic and political conditions improved, other European banks began to appear, and competition was intensified. But throughout, the aims of European governments were less important in the promotion of banks in South America than in the Near East.

Since the War the distinction between the international banks and the rest of the money market has been lessened, largely through the expansion of the activities of the large joint stock deposit banks; such changes were partly inspired by the Government during the war and after. At the same time the growth of nationalism, both in countries directly affected by the war and elsewhere, has tended to restrict the opportunities of the international banks.

From this historical study, set out in very clear terms, Mr. Baster concludes that the present position of these banks is not very happy. Their real and imagined connection with pre-war diplomacy has created a prejudice against them and they no longer have such strong influence where they operate. Nevertheless, they still possess a knowledge of foreign conditions which cannot be gained quickly, and a revival of international trade and investment will bring new business to them.

CHARLES H. WALKER.

Wages and Labour in the Lancashire Cotton Spinning Industry. By JOHN JEWKES and E. M. GRAY. Manchester University Press. 1935. xiv + 222 pp. 8s. 6d.

Since the days of Stanley Jevons, the Economics Department of the University of Manchester has rendered valuable service to society in Lancashire and beyond by its continuous research into current economic problems and the resultant stream of wise counsel upon matters of immediate policy. Professor Jewkes and Mr. Gray fully maintain that enviable reputation by the publication of the present volume. Workers and owners in the Lancashire cotton industry should congratulate themselves upon having such well-informed counsellors available for consultation in Manchester: the rest of us will be more cautious in our criticisms and generalisations concerning that industry if in future we maintain accord between them and the important facts revealed by this thorough and careful study.

For the settlement of wages in mule spinning the Lancashire workers have grouped themselves in the provincial and district organisations of the Amalgamated Association of Operative Cotton Spinners and Twiners. Six separate wage lists are in force in different regions, but two of them (those of Oldham and Bolton) govern the wages of about 85 per cent. of the mule spinners. The Oldham list in particular has been adopted in many other districts, and the lists of Ashton, Hyde, Preston and Blackburn do not apply by any means universally in their own localities. In addition, special lists are preferred and are not infrequent in particular mills. No wonder: the relative prices of the various operations scheduled in the different lists have remained substantially as they were in the '70's and '80's when the negotiators noted them down in the lively labour market of that time and embalmed them in these agreements. The Oldham list alone has been changed once in any significant detail during the present century, and it has tended to be preferred to the others. Textile machinery makers have not been idle in the last half-century, labour has not remained static nor is the market for cotton goods precisely what it then was. But the collective wage negotiators who should have been continually adjusting their relative prices to changing needs most certainly have been idle, and much of the mule spinning industry is in the doldrums. In ring spinning, the vast majority of firms have since 1912 operated a Universal list then established by negotiation with the workers organised in their Amalgamated Association of Card, Blowing and Ring Room Operatives, and once again other schemes of payment are also in use and yield higher pay. The ring list is thus not so far out-of-date, and ring spinning is relatively prosperous. Professor Jewkes and Mr. Gray go into the various lists in great detail, and emerge very anxious for the future of mule spinning in Lancashire.

What should be done, and what can be done? For all this time, Lancashire folk who have wanted to spin cotton yarn on mules have been confronted by a narrow range of obsolete wage lists which have placed severe limitations upon the range of practicable production in the peculiar market conditions obtaining in each locality. It is as though a population of varied stature were all trying to be well-suited by one of six mass-production tailors, each of whom offered only one size which happened to fit a particular client fifty or sixty years ago. In such conditions it would be little short of a miracle if a present-day Lancashire lad won a prize in a world competition for the best-dressed entrant. And that is what mule spinners in Lancashire have been trying to do during all these years. If the aim is that every person, from the finest to the meanest, is to enjoy the best possible employment fit, then there should be a vast increase in the available selection—in fact, bespoke tailoring is the goal to aim at. And in mule spinning the special lists operating in particular mills, and making output possible there, are then a step in the right direction.

It says a great deal for the scrupulous impartiality with which the authors have set out the facts that this inference can be drawn from their study, for Professor Jewkes and Mr. Gray themselves arrive at a precisely opposite conclusion. It is the main recommendation of their book that one uniform mule list be applied throughout the industry, or if that be impracticable, that the Oldham list be adopted wherever the Bolton list does not apply. All mule spinners are, as it were, to wear one of a maximum choice of two suits, the measurements of which were adapted to meet, as a compromise, the need of a past generation.

Everyone will appreciate the attractions of uniformity to a government conciliation officer, a social investigator, a trade association organiser or to firms which are favoured by the wage-rates. What is less clear is the social case for imposing upon minorities (p. 148) an arbitrary increase in costs which would contract still further the output and employment of the cotton spinning industry. A uniform list would " tend to eliminate the weaker technical units in the industry more rapidly than at present " (p. 149). Why not rather allow them to continue to provide work for those who prefer existing wages to unemployment ? " It is, of course, not a good thing for an industry as a whole that the inefficient areas should be able to maintain themselves at the expense of the efficient by paying lower wages, but undoubtedly it is a factor which can seriously impede movement to areas where methods of production are most up-to-date " (p. 114). What kind of " inefficiency " is this ? Are those workers who can earn least to be denied any work at all ? The case for coercion by majorities has to be carefully argued in each instance : here collective bargaining assumes almost the status of an end desirable in itself. In ring spinning, regional differences in wages before collective agreements were made are described without evidence as " quite fortuitous " (p. 33). Fluctuating wages " hardly seem desirable either on economic or on general social grounds " (p. 124) even though the variations are upward from approximately equal minima (tables on pp. 125 and 126).

In mule spinning, the big piecer and little piecer who assist each " minder " (spinner) are virtually employed by him, although the Bolton and Oldham lists prescribe the wage rate which he shall divide between them. The depressed condition of the industry has virtually converted these hitherto " improver " positions into blind-alley jobs, and growing numbers of piecers who in earlier days would have been promoted to minders' work or been attracted to outside posts remain at low wages as married adult men. The authors make a number of important suggestions for reorganising methods of recruitment and improving the distribution of duties and earnings on the machines, and hint in addition that the introduction of women piecers might ultimately provide the best solution to the problem. Is the strain of piecers' work so much less than that of minders that what has been impracticable in the past for the one (pp. 12–13) is reasonable in the future for the other ? Or has collective bargaining once again been an obstacle ?

Our debt to the authors is great. It is no mere Irishism to say that without the invaluable aid of this book the present somewhat captious review could never have been written.

Arnold Plant.

The Agricultural Dilemma. A Report of an Inquiry organised by Viscount Astor and Mr. B. Seebohm Rowntree. P. S. King and Son, Ltd. 1935. xiii + 100 pp. 2s. 6d.

This book, which constitutes the report of an enquiry, organised by Viscount Astor and Mr. Seebohm Rowntree, into the desirability of alleviating unemployment by the extension of domestic agriculture, deserves serious attention. Unlike the great majority of works on the so called " agricultural problem," it goes to the root of things, depicts agricultural depression in part as a phenomenon of economic progress, in part as a result of agricultural protectionism, and points to the strong probability that in future, as a result of the slowing down of the growth of populations, " a diminishing number of persons will be required in the food producing branches of agriculture taking the Western world as a whole." This broad view of the matter enables its authors to show the essential absurdity of the supposition that, in the extension of this industry, of all others, lies a sensible solution of any of the more important problems at present confronting the world. They show too, with a wealth of detailed information not easy to obtain concisely elsewhere, the extreme practical difficulty of any extension of agrarian protectionism in Great Britain, the probable repercussions on imperial relations and our export trade with centres such as the Argentine. All this is of course very well known to economists. But it is well that it should be said again and said with the authority of the members of this committee and the great force and eloquence of the expert draftsmanship of Mr. Hubert Henderson. It is, however, to be regretted that, having dispelled so many illusions, the committee should have permitted themselves to coquette, however cautiously—and they are very cautious—with the latest plan for maintaining agricultural capital values, the subsidisation of consumption. Surely, if it is thought that the money incomes of the poor are too low, then a simple transfer of money via the tax machine is appropriate. If it is thought that the price of food in particular is too high, then it could be lowered substantially by allowing it to be bought in the cheapest market. While this is not done, the device of subsidising consumption is merely a façade to make respectable the real subsidy to immobile capital and labour which is implied by agrarian restrictionism.

LIONEL ROBBINS.

The History of Milk Prices: An Analysis of the Factors Affecting the Price of Milk and Milk Products. By RUTH L. COHEN. The University of Oxford Agricultural Economics Research Institute. 1936. 205 pp. 5s.

Economists and students of marketing will be grateful to Miss Cohen for this very useful study of the factors affecting the price of milk and milk products during the last thirty years. Apart from the opening chapter which summarises what is known about the demand for milk and milk products, the book is divided into four main sections covering the pre-War period of individual contracts, the period of War time control, the post-War period of the Permanent Joint Milk Committee Agreements and the first year of the Milk Marketing Scheme.

Since transport costs are higher per gallon of milk equivalent on liquid milk than on liquid cream and on liquid cream than on butter or cheese, consuming centres tend in competition to obtain their supplies of liquid milk from surrounding areas and their supplies of butter and cheese from more distant regions. There will thus be a series of market areas. The most important cheese- and butter-making districts nearest to London fall within the 100–150 mile railway zone. In this zone therefore milk should realise as much net if it is sold for liquid consumption in London as it would if converted locally into butter or cheese. Miss Cohen shows that this was approximately true in the pre-War period of competition. An increase in the "liquid" price in London would extend the area from which milk for liquid consumption would be brought, and a fall in price would contract this area. It is therefore not clear why Miss Cohen thinks that if prices had been left to free competition after 1929 "it is probable that prices would have fallen further than the permanent situation justified and possibly that subsequently an actual shortage of liquid milk might have occurred (p. 142). A competitive price for milk would have reduced production mainly in the cheese- and butter-making areas, which at present can compete with low-priced imports only with the help of the Inter-Regional Compensation Fund and the Government guarantee of a minimum manufacturing price. A reduction in the liquid milk price would not have affected all producers equally, so that the danger of a shortage of liquid milk does not seem to have been serious. In addition, as Miss Cohen points out, "a substantial portion of producers might have been able to remain in business at much lower prices" as a result of the decline in costs which had taken place.

Miss Cohen shows that although the Permanent Joint Milk Committee Agreements were introduced in 1922 the price of milk was not seriously monopolistic before 1930, for milk made into butter and cheese realised approximately the same as milk sent liquid to London in those areas where milk was commonly supplied to both these outlets. With the rapid fall in butter and cheese prices after 1929 as a result of increased imports the manufacturing milk price fell, but the liquid price was maintained. It was to prevent milk from being diverted from the low-priced manufacturing market to the higher-priced liquid market that the Milk Marketing Act was introduced in 1933.

Miss Cohen makes clear the nature of the economic waste involved in selling milk for manufacture into butter and cheese at 3½d. or 4d. and milk for liquid consumption at 1s. 4d. She suggests, however, that, since the demand for liquid milk by the poorer sections of the community is very low and fairly elastic, while the demand by richer people is probably inelastic, it would be socially desirable and at the same time profitable to the Milk Board to charge poorer people lower prices than richer people. If it is assumed that there will be no reduction in the general retail price of liquid milk it would certainly be socially desirable to differentiate between different liquid milk consumers where this is possible; for this would mean diverting milk from the low-priced manufacturing markets to a higher priced market. But this further differentiation would be desirable only as long as the price is not raised in the higher priced retail market and must be regarded as a very poor substitute for the equalisation of the liquid and manufacturing prices.

R. F. Fowler.

Earnings of Skilled Workers in a Manufacturing Enterprise, 1878–1930. By E. B. Alderfer. Philadelphia: University of Pennsylvania Press. 1935. xi + 88 pp. $1.50.

This book consists of a short study of the wages paid in a particular occupation in one firm over a period of about 50 years. The company is in the textile industry and the occupation selected is one in which the technique of production did not alter, except for a minor change in 1900, over the period considered. The workers concerned were immigrants. The job is considered the most skilled in the trade, but is laborious and the conditions of work unpleasant. The main topics dealt with are the fluctuations in employment; the trend of earnings; the inequality of earnings within the group; the relation between age and earnings.

Although the facts are set out clearly, their analysis and the provision of supplementary information necessary for their interpretation seems much less satisfactory. Partly, no doubt, this is due to the desire on the part of the firm concerned that its identity should not be disclosed. Where there are divergences from what might have been expected from a knowledge of general economic conditions in the United States, this is described in the manner indicated by the following quotation:—"In 1903 for the first time the earnings of journeymen were in excess of $500 per year, but an immediate decline set in which persisted for three years. Paradoxically this was a period of great prosperity for the country at large. An even greater contradictory development occurred in 1907 . . . . ." No explanation is attempted of these and other divergences noted except that the tendency for changes in earnings to lag behind changes in general business conditions is discussed. This discussion is however brief and not very satisfying. No consideration is paid to changes in piece-rates. The discussion of the non-coincidence of the indices of production and employment left the reviewer in considerable doubt as to the probable explanation. In general, where an explanation is attempted, it is one congenial to common-sense. The greater labour turnover in times of prosperity is explained by the greater willingness of workers to try other jobs when the demand for labour is expanding. The greater inequality of earnings in times of prosperity is explained by pointing out that in a depression the less efficient workers will be dismissed.

Mr. Alderfer considers his "most startling discovery" to be the extremely short duration of maximum earning power. He finds that journeymen starting in this occupation at 21 take six years to reach maximum earnings, but that earnings start to decline when they are about 30 or 31. This result, however, is not based on very satisfactory data. The variation of earnings with age is only obtained for workers who continued to work with that firm. The duration of employment was, however, in general short—about half the journeymen remained with the company for a period of less than five years and about 70 per cent. for less than ten years—while, as Mr. Alderfer points out, it was the more efficient workers who left. The sample cannot therefore be considered to be at all representative.

R. H. COASE.

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Changes in the Price Level Under the Influence of Maladjustment of Supply and Demand

By Wl. Zawadzki

There is much disagreement among the theorists in answering the question of how the general level of prices would behave if changes on the money side were abstracted from. The so-called compensation theory claims that when the price for one good falls as a result of discrepancy between supply and demand the price of some other good rises correspondingly, the two changes thus offsetting each other and the general level of prices remaining unchanged. The same idea is to be found at the basis of all the attempts of measuring changes occurring on the money side by means of index-numbers. According to another theory—the theory of cumulation—any change in the price of one good means a change in the purchasing power and hence also in the demand of sellers of that good, and thus entails changes in prices of other goods in the same direction. Both the above explanations are rejected in some recent theories,[1] but no positive conclusions are supplied instead.

I believe that in studies on this subject one should be careful not to start—as is often done—from (1) considering changes in supply of and demand for some good as independent from other changes, and (2) assuming that the purchasing power available on the market is some definite quantity which can be shifted from one kind of goods to another.[2]

These assumptions are erroneous, although in individual cases they may fit the actual conditions fairly well. In fact, it may occur that the effects of a certain change will have neutralised some other change before it had time to make itself felt. We then may have the impression of dealing with the same purchasing power shifted in both directions. But in the economy as a whole the purchasing power does not exist as a definite quantity; it is created again and again in mutual transactions. If we adopt the two above-mentioned starting points we have to rely on arbitrary assumptions as to the succession of changes. This is avoided if we consider shifts in adjustment of supply and demand for one good as being closely connected and simultaneous with shifts in the opposite direction occurring with respect to some other good. Of course, to get a true insight into the actual course of events we have to take their succession into account, but then changes on the money side are to be taken into account, too. Now, if our wish is to examine one side of the phenomenon in its pure form, we must remove all the factors that might, with equal probability, give rise to deviations in either direction. However important those secondary influences may be in the actual world, they ought not to be included in the argument too early, lest the definite question should remain unanswered and the false conclusion be drawn of any solution being possible.

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[1] Koopmans: "Zum Problem des 'neutralen' Geldes," in *Beiträge zur Geldtheorie*, ed. by F. A. Hayek, Vienna, 1933. *Ib.* a detailed bibliography of the subject.

[2] An assumption of this kind is legitimate only if we admit that in each period the amount of money to be disbursed on purchase of goods is determined regardless of receipts of individuals. But, as will be made clear at a later stage, such an admission would contradict our statement that there are no changes on the money side.

Accordingly, I shall begin with setting out the basic assumptions which will underlie the whole of my argument. Their relation to the actual conditions will be discussed later. I shall assume that all the goods are exchanged for money, while all the transactions take place simultaneously, so that their terms are fixed once during a definite period, the same for every kind of goods. Next I shall assume that the only source of means for buying goods are receipts from selling those very goods, and that the purchasing power which is an outcome of such transactions is the only purchasing power available. To put it another way, I assume that cash balances in the hands of the public are not being reduced, and that money taken to finance current payments flows back to the reserves at the end of the period. I also lay down that each seller expends all the money he receives on the purchase of goods, and that, accordingly, the cash balances are not increased either. These statements are logically derived from the assumption that there are no changes on the money side. Last comes the condition that the full supply of goods offered for sale must be cleared, that no goods can be held back for speculative purposes, and that there are no inventories of unsold goods carried over from the preceding period. I shall thus examine what would be the changes in the price level entailed by a maladjustment of supply and demand under the above conditions.

It may be well to determine what the circulation of money would be in the light of the above hypothesis. Since we deal with a monetary economy, money is used to make payments; and, of course, it is taken out of the available cash reserves, but taken for a temporary use, *i.e.*, in a sense borrowed. Further, we have to admit that the cash balances are large enough for enabling each dealer to meet his successive payments, lest the shortage of cash should compel him to sell at a lower price. But there is no need to suppose that each dealer has got an amount of money equal to at least the sum total of his purchases during the whole period. We can well imagine that, the price once stabilised, goods will be sold by lots, *i.e.*, that the same monetary unit will pass from hand to hand several times. Or else we can admit that all goods are bought and sold on account, and that eventually all the accounts are cleared, since everybody buys exactly as much as he sells; but this, of course, is a different way of saying that, eventually, each dealer will add to his cash reserves the very amount he had borrowed at the outset. In such conditions the actual volume of money in circulation is of no account really, and in no proportion to the volume of transactions; a relative excess or lack of money would have been immediately compensated by an inverse change in the velocity of circulation.

Thus, under the above conditions, money is a wholly neutral and passive element, with respect both to the relative prices of goods and to their prices in terms of money. Even if we had ascertained the amount of money in circulation this would not help us in determining the absolute level of money prices from our data.[1]

The thing that matters is not the absolute level of money prices, but their changes. In that respect the following postulate will be sufficient. If at a definite money price demand equals supply, the price remains unchanged; it rises if demand exceeds supply, and falls if demand runs short of supply.

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[1] Hereby I do not commit myself to any definite attitude towards the view that money prices in general cannot be derived from considerations of a static hypothesis. This might be possible if certain assumptions were made, but the question is beyond the scope of this study. See Rosenstein-Rodan: "The coordination of the general theories of money and price" in ECONOMICA, August, 1936; G. Myrdal: "Der Gleichgewichtsbegriff als Instrument der geldtheoretischen Analyse" in *Beiträge zur Geldtheorie*, Vienna, 1933. The first work contains a comprehensive bibliography of the subject.

It may also be followed from the above remarks that not every change in the volume of money is what we should call a change on the money side; for, in point of fact, money flowing to the economy may stick in cash reserves, and money withdrawn from cash reserves may be exported. This is true even of a change in the amount of money serving to meet current payments, if the assumption holds that the total outlay is based on current receipts. Unless such a change directly impedes current payments (*e.g.*, by depriving some dealers of means of making their first purchases), it affects the velocity of circulation only.

A typical change on the money side occurs either when some amount of money is brought to market which does not represent the result of a sale of commodities during the given period, or when receipts from a sale are withdrawn from market during the transaction. This may happen either as a result of some shifts in cash balances or as a result of an inflow of some new media from outside (imports of gold, creation of credit) or of a drain on the money resources of the economy (exports of gold, contraction of credit, also repayment of debts). Without going into a detailed analysis, I should only remark that each of the two categories of changes is of a different consequence, and in the first place that their effects are not equally durable.

Theoretically, there might be still another change on the money side, notably a change in the valuation set on money, *i.e.*, in the notion of value associated with a certain amount of money. Here again an analysis would lead us beyond the scope of the present study. Assuming that the idea stands the test (and I do not see how it would be possible to reject it *a priori*), the effects of such a change would most frequently be repercussions on cash reserves, but it is quite possible that there would also be some changes in prices both charged and offered. Here undoubtedly belongs the case where, because of the shortage of cash, some dealers must so arrange their marketing that they get the necessary minimum of cash with the shortest possible delay. I thus shall assume that changes of this kind do not occur during the transaction.

When comparing our assumptions with the conditions prevailing in real life, it is necessary to abstract from the question of possible changes in money resources of the economy as a whole, for these changes are not connected with its normal functioning. As regards other points, our assumptions diverge from reality in that we have laid down that all the exchange operations are simultaneous, that cash balances in the hands of the public do not change, and that each dealer is sufficiently provided with means of payment. Meanwhile, in the actual world transactions in individual commodities are by no means simultaneous; money being taken out of cash reserves to provide for income deficiency at some periods and its being withdrawn from circulation to complete cash reserves are in the very nature of monetary economy; and dealers' attitude during transactions is greatly influenced by the shortage or abundance of their resources as compared with their requirements. As a result of all these, the real course of any event must diverge from our model, while deviations in either direction are possible. It may be assumed that sometimes those additional influences can offset—and so they actually do—the effects of slight and short-lasting discrepancies between supply and demand. But they disappear behind durable and serious shifts in the demand. In the long run current income is undoubtedly the only source providing for the purchase of goods, and on the other hand practically the whole of it is eventually expended on purchase of either consumers' or investment goods. The above assumptions thus express the long-run tendencies at work in the actual world.

II

I shall proceed now to the actual object of my study. Imagine that the owners of all the goods to be exchanged gather in the market. Each seller has some idea of the price he hopes to get for his goods, as well as of the price he will himself have to pay for other goods; and he schedules his own demand for other goods accordingly. We have there what I shall call the initial price of the transaction, which can—but must not—be equal to the price paid of late for the given kind of goods. Thus, we shall take as the starting point the money price expected by each seller and, accordingly, we shall mean by initial demand a demand that would exist at money prices expected by respective sellers. In other words, we shall examine changes in the level of prices not in comparison with some previously existing price level, but in comparison with a price level that would obtain if each seller were able to sell at the price he expects to reach. It is obvious that, unless a definite and uniform pricing system is adopted, the whole setting of the problem is deprived of its real sense.

The addition of individual dealers' demands for a given good yields the volume of total demand for that good both in terms of physical units and in terms of money. Now, the sum total of all the individual demands (in terms of money) for all goods must be equal to the sum which all the sellers together expect to get by selling all their goods. But the volume of demand thus ascertained is not the ultimate one, unless actual prices live up to sellers' expectations, *i.e.*, unless the quantities supplied of each good happen to correspond exactly to the demand for the same good at the initial price. When there is no equilibrium between supply and demand prices change; sellers then are unable to receive the amounts of money they expected; accordingly, they have to change their own demand for goods they intend to buy, and this in turn must affect prices for those goods. The mutual repercussions of demand upon prices and of prices upon demand will go on until such prices obtain at which the demand for and supply of each good equal one another, and so do the total volume of demand in terms of money and the total receipts from selling all goods. The question which interests us is what will be the relation of that new price level to the one corresponding to initial prices.

There are many possible combinations of shifts in demand with respect to supply, but in the light of our assumptions (in particular of that postulating that all the money received should be spent on the purchase of goods) they always assume the form of an excess of demand over supply of some good or goods, and of a simultaneous excess of supply over demand for some other good. Let us call A goods supplied in quantities exceeding the demand for them (*i.e.*, goods whose price is falling) and B goods enjoying a demand which is greater than their supply (*i.e.*, goods whose prices are rising); all the other goods, *i.e.*, those the initial demand for which equals supply shall be denoted by C (or C_1, C_2. . . .). The above simplification is justified in so far as a further differentiation of goods would always lead us to one of those simplest cases. We shall thus assign various goods to the same class if they enjoy, broadly, the same conditions of supply and demand. The classes of goods being so defined, the following combinations are possible:

(1) The supply of all goods is either less or greater than the demand they enjoy. In this case there are only two classes of goods on the market: A and B; the scope of each is very wide, and we have to suppose that sellers of A and sellers of B are at the same time buyers of goods belonging to their own group.

(2) Besides A and B there are also goods C whose supply equals the demand (at least at the outset). Then sellers of A and B must not be buyers of goods belonging to their class, and I shall regard as typical the case when they are not. But sellers of A are buyers of B and *vice versa*.

(3) Position analogous to (2), but no exchange between A and B.

Let me begin with (1). I shall suppose that there are only two classes of goods on the market—A and B—*i.e.*, that initially all goods are supplied in quantities either excessive or deficient with respect to quantities demanded at the existing price. Goods are being exchanged both among these two groups and within them. Now suppose that we have determined some natural units of goods belonging to each class in such a way that the initial price of each unit is equal to 1.

Let us consider what would happen if there were a barter system. At first sight this seems to depend on the elasticity of demand for each class of goods. This is not quite true, but the argument will be made easier if we examine successively cases where the elasticity of demand of the second group for $A = 1$, < 1, and > 1.

(1a) In the first case the situation is as follows: sellers of B decide (the price of A in terms of B being equal to 1, for we are now dealing with prices in kind and not in money) to keep a quantity of B equal to $1 - x$ and to exchange for A a quantity equal to x. But the demand of sellers of A for B is $> x$, thus they are ready to lower the price in order to get the quantity desired.[1]

The price falls, but this only calls forth an increase in quantities of A received by the sellers of B who, with an elasticity of demand $=1$, continue to offer the same quantity of B for the ever increasing quantities of A. This process will go on until, as a result of a change in marginal utility (or in some other factors controlling the demand—if the theory of marginal utility be rejected), a price obtains at which the group A considers unprofitable to increase any further quantities of A offered for a unit of B. What that price will be like depends on the elasticity of the demand A for B, but this circumstance is of no account. The important thing is that the equilibrium will be established at a price of A in terms of B (p_{ab}) that will be less than 1, while the group B will give up a quantity of B equal to x and keep a quantity $1-x$ as initially intended (the first group keeping either as much, or more or less than was initially intended).

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[1] This would not be the case only if A were completely monopolised, but then there would be no excessive demand either.

But this applies to the price in kind, which does not tell us anything about the money price. Let us now resume the hypothesis of all the exchanges being carried out in terms of money. At first sight it might seem that the same result is obtained whether through raising the money price of B or through lowering that of A. Indeed, $p_{ab}=\frac{1}{2}$ ($p_{ba}=2$) may exist with money prices 1 and 2 as well as with money prices $\frac{1}{2}$ and 1 or any other prices whose ratio would be 1 to 2. This might be true if sellers of B were not buyers of B at the same time. As it is, with an elasticity of demand $=1$, the equilibrium must be reached at the point at which the sellers of B get an amount corresponding exactly to their initial demand. But then—unless something has unexpectedly altered their attitude in the meantime—there is no reason why they should be willing to pay for B (in terms of money) more than before, *i.e.*, more than 1.

Eventually, the equilibrium is established in a point at which the demand for both goods equals the supplies of those goods on the basis of a price of A < 1 and of a price of B $= 1$; in this case the price level is lowered, and this under a purely static hypothesis, without any monetary factors being at work.

(1b) The above conclusion was drawn under the assumption of the elasticity of demand of sellers of B for A being equal to 1, and one might think of this assumption as of a conditioning factor. But it hardly needs any further explanation that our conclusion will hold *a fortiori* if we suppose that the elasticity of demand < 1, *i.e.*, that increasing quantities of A can sell only at a price lowered more than in proportion. In that case the group B would not even have to give up the whole quantity appointed beforehand (and then the price of B, too, might fall).

(1c) More complicated is a case where the elasticity of demand is > 1. Our previous argument can hardly be applied now, for we do not know what will be the quantity of B kept by the members of the second group as compared with $1-x$. We thus must approach the problem from the side so to say, and see what would occur at the extreme points of elasticity of demand, taking for granted that intermediate cases will also yield intermediate solutions. Those extreme points are, on the one hand, the elasticity $= 1$ and, on the other, the elasticity $= \infty$, by which we mean that the least deviation of price immediately raises the demand up to the maximum limit of the available supply. We have seen that with an elasticity of demand $= 1$ the general price level declined by as much as the price of A did. We may suppose that with an elasticity of demand approaching but > 1 the effect would be similar, though less drastic. The further we get from equality to 1, the greater will be the deviations from the case described under (1a). The question may arise as to whether a reversal will not appear at the end. I do not suppose it would. For the elastic demand for A on the part of sellers of B corresponds to the equally elastic demand for B. If the elasticity were equal to ∞, the least decline in the price of A and the least advance in the price of B would be enough to restore the equilibrium immediately. Only in this case—which, of course, is purely theoretical—would the price level remain unchanged. When the elasticity is less than ∞, there is a minimum decline, growing ever greater as the elasticity is falling. Thus, here again the price level[1] must be lowered, as a rule.

(2) Now suppose that beside the groups A and B there is on the market the group C; in such a case sellers of A and B must not buy their own goods, and we shall suppose that they actually do not. Let me try to show first what would occur under conditions of a barter.

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[1] Of course, we have to take into account the importance of individual prices which depends on the volume of transactions in the given kind of goods. This means that our price level corresponds to the weighted index of individual items.

We shall find, as we did before, such units of each of the three classes of goods as to make the initial price equal to unity. Then $p_{ab}=p_{ac}=p_{bc}=1$ (and also $p_a=p_b=p_c=1$).

On the basis of these prices dealers schedule their demand as well as their supply. We thus obtain:

Demand on the part of A for B—x' supply being equal to x'
” ” ” ” ” A ” C—y' ” ” ” ” y'
” ” ” ” ” B ” A—x'' ” ” ” ” x''
” ” ” ” ” B ” C—y'' ” ” ” ” y''
” ” ” ” ” C ” A—x''' ” ” ” ” x'''
” ” ” ” ” C ” B—y''' ” ” ” ” y'''
Demand for A—$x''+x'''$, supply of A—$x'+y'$
” ” B—$x'+y'''$, ” ” B—$x''+y''$
” ” C—$y'+y''$, ” ” C—$x'''+y'''$

Ex hypothesi we have

$$(x'+y')>(x''+x''')$$
$$(x'+y''')>(x''+y'')$$
$$y'+y''=x'''+y'''$$

Let us suppose further that the first two inequalities are also true for the component parts, *i.e.*, that $x'>x''$, $y'>x'''$, $y'''>y''$; then prices p_{ab}, p_{ac} and p_{cb} are falling (prices p_{ba} and p_{bc} are rising).

Let us consider first a case where the elasticity of demand of A on the part of B and C is equal to 1. Then p_{ab} will decline in proportion $\frac{x'}{x''}$, and p_{ac} will decline in proportion $\frac{y'}{x'''}$. Sellers of B and C will get the whole quantity of A in exchange for only such quantities of their own goods as initially appointed. It is obvious that, as a result, there will be a decrease in the demand for B and C taken together. But it is not at all obvious why the higher price of B should not bring about an additional demand for C on the part of sellers of B, which would make up for the loss of sellers of A as customers. Yet, it cannot possibly do this. In fact, the price of B (in terms of money) can remain >1 after the decrease in the demand of the group A only if $p_{bc}>1$, *i.e.*, only if, at a money price of C$=1$, y''' remains $>y''_1$ (even if y'' should change under the influence of a higher price for B). But the price of A being altered, we have at the same time

$$y'_1=x'''$$

and thus the equation

$$y'+y''=y'''+x'''$$

ceases to exist and is transformed into the inequality

$$(x''' + y''') > (y_1' + y_1'')$$

where y_1 stands for the demand after the change in prices. But the demand for C could not possibly decrease without a fall in p_c, *i.e.*, in the money price of C, which fall would in turn lower the general level of prices. Or else we could admit that the existence of an excess of supply over demand automatically shifts the group C to the group A, and then the whole problem may be classed under (1).

We might obtain the same result directly assuming that the initial elasticity of demand for C on the part of sellers of B is equal to 1 and complicating the problem as we did in the preceding case. Here again an argument analogous to that under (1b) and (1c) will enable us to extend our conclusions to cases where the elasticity of demand for A on the part of sellers of B and C is less than unity, greater than unity, or different for each group. And, last, if we suppose that of the three inequalities:

$$x' > x''$$
$$y' > x'''$$
$$y''' > y''$$

only one or two are satisfied, the argument is very much simplified, as may be easily seen.

(3) There is no exchange between the owners of A and B (*i.e.*, prices p_{ab} and p_{ba} do not exist at all); the price of A is falling, and the price of B is rising; both change in the same proportion. In that case the price level is kept stable for a certain time. The increase in money receipts of sellers of B raises their demand for C by the same amount (in terms of money) by which the demand of the group A was lowered. In the event of both the increase and the decrease in demand concerning the same goods C the price level may be kept unchanged. But in the event of an increased demand for some goods C_1 and of a decreased demand for some other goods C_2 further deviations must occur which will fall under one of the already discussed headings, and accordingly will either let the price level remain unchanged or lower it.

It remains to add that if prices change only with respect to goods A and B the elasticity of demand must be the same on the average for either of them; otherwise changes would automatically run through to goods C, *i.e.*, we should again have one of those cases dealt with in the preceding points.

On the ground of the preceding argument the following general statement may be formulated. Under the assumptions set out at the beginning of the present paper, and abstracting from changes on the money side, *the maximum price level will be obtained in the event of supply and demand being equalised on the basis of the initial price of the transaction, i.e., of the expected price upon which the demand schedules were based.* If demand fails to fit the available supply at that initial price, then, as a rule, the price level is lowered ; only under special circumstances can it be kept stable, and it never can be raised.

III

The above result might at first appear paradoxical, if not absurd. It seems to suggest that (when there are no changes on the money side) changes in the price level must always be in a downward direction, there being no possibility of any upward movement, or even of a return to their previous state. Does not this contradict the whole of our experience ? Now, in the first place, we do not know from our experience such cases where changes on the money side could be assumed non-existent, but even aside this, the above conclusion does not follow from our preceding thesis. I have tried to call the attention to the difference between prices actually reached and those expected by sellers (*i.e.*, those on which sellers base their demand schedules) which is an essential point in my argument. Most often the expected price will be that resulting from the very last actual transaction, but this must not necessarily be so. If, *e.g.*, there is ground for considering some recent price decline as exceptional, sellers may well base their calculations on prices quoted in any earlier transaction, and then nothing will prevent them from returning to the preceding price level. They also can schedule their demand on the basis of a higher price, when there is some reason for foreseeing an advance, and if the actual demand happens to live up to their expectations, the price level may become higher than it was before (while not higher than expected). Thus, it does not in the least follow from our thesis that under the influence of maladjustment of supply and demand the price level must be steadily declining.

The above explanation does not refute the charge yet.

The following reasoning would be justified: the question of the expected price may be indifferent; let us even suppose that sellers do expect the lowered price at which commodities had been able to sell in the last transaction; supposing they none the less adapt their supply in the next period to the demand that existed in the preceding period before the price declined (and was *ex hypothesi* non-adapted to the respective supply), we come back to the price level previous to the slump. This conclusion, while obvious at first sight, is only true with respect to relative prices of goods, not to their prices expressed in money.

Let us thus suppose that in some period we had, at a price 1 (as above) a demand x for goods A and a supply y of same goods (*ex hypothesi* $y > x$). At the same time we had a supply y' of goods B and a demand x' for those goods, while $x' > y'$. With an elasticity of demand on the part of sellers of B$=1$, the price of A was bound to drop below 1, the price of B remaining equal to 1. Now, let be in the next period the expected price $p_a < 1$, the expected price $p_b = 1$, the supply of A$=x$ and the supply of B$=x'$. We then shall have at the same price a demand for A $> x$ and a demand for B $< x'$, *i.e.*, a discrepancy between supply and demand, and hence—in conformity with our previous reasoning—a further lowering of the level of money prices. And then why should prices not return to the level 1, since at that level the demand was adjusted to supply?

There is no doubt that in such conditions prices of both classes of goods must become equal again, *i.e.*, that we shall have $p_{ab} = p_{ba}$ and $p_a = p_b$ (in terms of money). But supposing that sellers of those goods have taken the previous change seriously, and have scheduled their demand on the basis of $p_a < 1$ and $p_b = 1$, the equality can by no means be reached at a level $p_a = p_b = 1$.

It is true that at a price $p_b = 1$ demand runs short of supply, and that the price of B must then fall both in terms of A and in terms of money. As, eventually, the price of B must become equal to the money price of A, that price, too, must be lower than 1. It will depend on the elasticity of demand for B on the part of sellers of A whether the price of A will remain on the lowered level or will rise. But even if it does rise, it may—in the best event—be able to keep the general price level in proximity of the previous (*i.e.*, lowered) level, but never to raise it.

The above argument relative to prices of two goods and to an elasticity of demand equal to 1 may be extended to all the other cases, similarly as this was done in Part II.

The position would probably be different if sellers of both goods regarded the decline in the price of A as temporary and somehow abnormal, and continued to reckon with the previous price $p_a = 1$. In such a case the making of the supply of A equal to x and of the supply of B equal to x' would necessarily lead to an equalisation of prices on the level 1. This would represent a rise as compared with the previous price level, but not as compared with the expected price level, while supply would be exactly adjusted to demand at the expected price.

One might be surprised at seeing that the same relation of supply and demand can yield two different combinations of money prices according to the point we start from. But this is neither absurd nor paradoxical, and consistent with the assumption of there being no changes on the money side and with the passive part played by money in such a system. The problem has already been dealt with in Part I. In such conditions the influence of money is only revealed through the association in dealers' minds of a definite money price with the notion of a definite amount of value; thus, it should be easy to understand how the final result may be influenced by the fact of these and not other money prices being chosen as the starting point. It could be otherwise if dealers' ideas concerning the value of money were subject to changes, but this is the very thing we decided to abstract from.

The expected prices may correspond to the existing prices, but they may as well correspond to the money cost (calculated in one way or another, even slightly augmented). In the last event, to bring prices up to the previous level would be a rather easy matter (assuming that the money cost does not change in the meantime). Then the main thesis could be formulated as follows:

Assuming that there are no changes on the money side, the maximum level of prices is reached when all goods are *supplied in quantities equal to those which find buyers at a price corresponding to the cost.*[1]

But if we assume that the productive services whose prices determine the level of cost are also commodities exchanged on the same market according to our assumptions, then the amount of money cost will no longer provide a basis for determining the price level. On the other hand, had we assumed that it is possible for some element of cost (*e.g.*, wages) to advance regardless of the movement of other prices, the result would have been a rise of the general price level.

The adoption of initial prices that would be higher than the existing ones is always possible, and such an event could hardly be excluded *a priori*. However, as these are prices on which sellers base not only their expectations of receipts but also their demand for other goods, we may take for granted that they will not calculate at a higher price unless they are pretty sure of getting it. One could hardly imagine a dealer who would base his demand on a hope of reaching a price 2 when he is likely to get 1; the more so as such a hope would have to be shared by all the dealers. But of course in some cases there is sufficient ground for expecting an advance in price, and this may actually lead to a rise of the general price level.

The most important case[2] of that kind occurs when the excess of demand over supply is visibly concentrated upon some narrow sector of goods and when the corresponding excess of supply is being spread over the rest of the market in an invisible manner, in particular if supply is commonly known to be deficient before the beginning of the transaction. Then both sellers and buyers approach the transaction in the belief that the respective price will rise, and they base their calculations thereupon.

Not only are such cases possible, but they certainly occur with respect to goods whose supply cannot be easily increased. I believe that a more close analysis of such cases would throw some light on the problem—so often discussed in both English and American literature—of the influence of agricultural prices on the general price level, and in particular on the level of prices of industrial commodities.

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[1] The way in which cost is established is of no account here; as regards the price level, it will be the higher the more "liberally" cost is calculated, in particular the greater are the profits counted in. The only condition to be imposed is that the same way of calculating the cost should be applied to prices reached and paid.

[2] Another important case would be an advance in wage rates as a result of an increased pressure on the part of wage earners.

The analysis of those problems would extend beyond the limits of the present paper, but it remains to consider another aspect of the matter. An impression might arise that the case just considered abolishes to a great extent the results of my previous argument. Since in the actual world transactions are not being performed in closed periods, but go on continuously, each moment may be as legitimately regarded as initial. In case of some maladjustment of supply and demand sellers of B may well get aware of demand exceeding supply after the first transaction and set their calculation on a higher price basis; the price level then would rise, as it did in the last considered case. But there is a serious difference between a price advance resulting from a transaction and that being a mere anticipation of a real price advance. In the first case the higher price corresponds to some, however transient, equilibrium which involves a simultaneous decline in the price of some other good, and—according to our argument set out in Part II—also a decline in the general price level; an advance in the price for one good, however real, cannot make any difference. In the second case, the advance in the initial price of one good occurs without there being any fall in prices of other goods.

The case just dealt with thus is not an exception from the general rule, but its application to definite conditions. What makes all the difference is different initial price, not a different mechanism of price movements. In such a case an advance in the initial price is likely to occur, and there are economic reasons supporting it (like in the case of a price dropping below the level of cost); now, instead of being an outcome of an exchange transaction it is prior to it and that is the very reason why it is not bound to bring about a fall in prices for other goods and why it may call forth a general advance in the price level. Whereas, if the advance in the price of any good results from the mechanism of the transaction, there must be—according to our assumptions—a corresponding fall in prices of some other goods and hence in the general price level. Frequent cases occurring in the actual world which seem to evade the above rule are outcomes of such circumstances as the non-simultaneity of transaction periods, or changes in cash balances, or some other sources of additional influences, dealt with in Part I. If the argument is not cleared of these elements carefully enough, it may easily supply a motivation either to the theory of compensation or to that of cumulation, or to some other statement relative to the price level.

IV

In the actual world the price level is conditioned by factors representing very complex and steadily varying combinations of mutual influences of changes in the amount of economically active money, of valuations set on it, of anticipations with respect to individual prices, and, last, of shifts in the conditions of supply and demand. The results arrived at in the present paper are not directly applicable to the actual changes in the price level, they form but one item in the analysis of those changes. My aim was to throw some light on that precise point. Still it is possible, while combining the results reached in the present study with what is known about changes in economic quantities in general, to formulate some, while very approximate, conclusions.

(1) Assuming that media for buying every kind of goods are exclusively provided during a certain period by the receipts from selling those goods during the same period, and that, consequently, there are no changes in cash balances, not only may a maladjustment of supply and demand lead to a lowering of the price level, but such a lowering is to be considered as a normal outcome of maladjustment of supply and demand.

(2) The restoration of the old level of prices or any other change in an upward direction is possible (with the same assumptions) provided that prices on the basis of which the demand for all goods is being scheduled are those corresponding to the money cost, and provided that actual prices happen to be below the level of cost.

(3) If there is some serious and more durable discrepancy between supply and demand, the tendency of money prices to keep up with cost proves unable to prevent a general fall in prices of goods and services.

(4) The price level may also rise if dealers can correctly foresee the advance in prices of some goods and if they at

once adapt their demand schedules for all goods to that higher price.

(5) In many cases, however, the rise of prices can only occur provided that assumptions made at the beginning of the present paper are violated, *i.e.*, that there is demand having some other source than receipts from current selling operations. Such a demand can be brought about in the long run only through supplying the economy with some new money resources.

Thus, an increase in the amount of money can affect prices in two different ways: through making possible the expansion or creation of cash reserves according as the economy is expanding, and through offsetting price declines caused by maladjustment of supply and demand.

Some Technical Aspects of the Finance of Rearmament

By N. F. Hall

The object of this note is to discuss not the desirability of meeting part of the proposed expenditure on rearmament by borrowing, but some of the technical considerations which seem to have weighed with the Government in selecting the methods by which the additional money shall be provided. The Government has taken power to finance over a period of five years part of the expenditure of the Defence Departments up to a total of £400 millions by borrowing. The Departments are to pay 3 per cent. interest on the money loaned to them, and after their borrowing has been completed, the money will be repaid by instalments over a period of thirty years. Powers have also been taken to lend the realised surplus on the Budget, that is the Old Sinking Fund, to the Defence Departments, thus modifying the Exchequer and Audit Act of 1868 which required that all surpluses realised in this way should be issued to the Commissioners of the National Debt for the purpose of debt reduction. At the time of writing (first week in March, 1937), these two facts are all that is known of official intentions. In the light of recent financial history, however, it seems possible to deduce from the nature of the powers which the Government has taken, the probable character of its intentions.

One of the results of the reduction in the volume of impending maturities of relatively short term government debt since the great conversion of 1932 and of the improvement in the financial mechanism of the Unemployment Insurance Fund since the genuine balancing of the Budget in 1931, has been to increase the importance of the extra-budgetary funds which are administered by the National Debt Office on behalf of the public departments through which they arise. The existence of these funds which have accumulated substantial surpluses, means that a surprisingly large part of the total internal Debt of the Government is now under official control. At the end of 1935, the surplus of the National Health Insurance Fund was £133·4 millions, Pensions Fund £22·5 millions, Unemployment Insurance Fund £24·2 millions, total liability to depositors in the Post Office Savings Bank £390·3 millions, in the Trustee Savings Banks £197·4 millions, a total on these five funds alone of £767·8 millions[1]. In addition to these, there are a variety of lesser sums of money, the management of which is entrusted to the Commissioners of the National Debt, and if the securities in the Issue Department of the Bank of England are included, as may be justifiable since the Treasury secures the profit made on the note issue, the total is in excess of £1,000 millions.

Elsewhere[2] an attempt has been made to show how the existence of these large sums of money make it possible for them to be invested in a manner which can contribute to the general monetary policy which the Treasury is pursuing. Changes in the magnitude of these funds seem in recent years to have been not unassociated with the maintenance of "cheap money." For example, the surpluses of the five most important of them declined by slightly more than £50 millions in the years 1929–31 inclusive, and increased by rather more than £100 millions in the years 1932–34 inclusive. Since an increase in these funds ultimately involves a purchase of Government securities by the National Debt Commissioners, it can be seen that the general buoyancy of the gilt-edged market since 1932 can, in part, be attributed to them.

With the Treasury about to re-enter the market as a net borrower on all accounts, the position of the past few years is at once altered, but it appears that the powers which the Government has taken in connection with its rearmament proposals are so designed that the capacity of the Treasury to influence monetary conditions through the administration of these funds shall be only reduced by the smallest possible amount. In the recent past, the Treasury seem to have been able to choose for any conversion operation which involved the issue of a new security a time when either these funds held a substantial portion of the security which had to be converted, or when they were otherwise able to assist the market. The danger inherent in borrowing to meet rearmament expenditure was that the Treasury might have been obliged to go into the market at moments determined by the needs of rearmament expenditure and not by its judgment that conditions were favourable for a loan operation. The powers which have recently been taken reduce to a minimum the risk that the Treasury may be forced into the market under unfavourable conditions. They have the effect of isolating rearmament finance from general Treasury policy. In the first place, it is now possible to lend future surpluses of the extra-budgetary funds to the Defence Departments. The fact that the rate of interest has been fixed at 3 per cent. suggests that the Post Office Savings Bank, amongst other asset-holding departments, will lend to the Defence Departments as the need arises. The form of security which the Treasury will presumably issue to the lending departments in return for advances made by them is likely to be particularly suited to the requirements of the Post Office and Trustee Savings Banks and similar funds. It will presumably be an obligation charged on the Defence Departments to pay 3 per cent. interest and to repay capital over a fixed term of years. It will, in fact, be similar to some of the assets now carried by the two Savings Banks, and in particular to the obligations of the Unemployment Fund to repay advances received up to 1931. The two Savings Banks carry in their books part of these obligations of other government departments in a special form. They show not the present market value of their rights to repayments, because there is in fact no market for them; they show instead the "amount of principal in future instalments" or some similar phrase.

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[1] The several funds make up their accounts on different dates so this "total" is only representative.

[2] *Proceedings of the Manchester Statistical Society*, February 10th, 1937.

Under present day circumstances the Treasury may get a definite advantage by increasing the amount of the assets of this type held by the two Savings Banks, because if money rates rise, there can be no decline in the book-keeping value of assets carried in this form because the "amount of principal" to be repaid will not vary as it would in the case of any other form of security. The larger the portion of the assets of the two Savings Banks which are carried in a form which cannot decline in capital value, the smaller is the Treasury liability in respect of the management of the Savings Banks' funds if interest rates rise. Under present regulations, the Consolidated Fund must in each subsequent financial year make good any decline in the value of the assets held in the two Savings Banks below the total of their liabilities as shown at the end of the preceding financial year. If money is to be less cheap than it has been in the recent past, the Treasury may find itself obliged to make good book-keeping losses on these extra-budgetary funds in addition to paying higher interest on the floating debt. If it is correct to assume that the defence annuities will be held by these extra-budgetary funds, the Treasury is effectively hedging against a part of this liability in the future.

The extra-budgetary funds will be able to find part of the money necessary to purchase annuities from the Defence Departments from any increase in their surpluses during the next five years. It is unlikely that the rate of increase of the past three years, which has been approximately £50 millions a year, will be maintained, but assuming active employment and relatively high money wages, there is likely to be some continuing surplus available. There will also be the Old Sinking Fund surplus which could be increased if the Chancellor and the House saw fit to do so, by carrying into the Budget under Miscellaneous Receipts in each of the five years a partial interim dividend on the revaluation of the gold holdings of the Issue Department of the Bank of England and of the Exchange Equalisation Account. These " profits " could be used to purchase securities from the Government Departments now holding them, enabling these Departments to lend to the Defence Departments. Only after these possibilities have been exhausted will it become necessary to sell in the market part of the government securities now held by the extra-budgetary funds in order to enable them to take up Defence Departments' annuities. It is possible that these funds may have already taken some of the profits which have accrued on their holdings of long-term securities by selling before the rearmament plans were announced. If they have taken profit in this way and have got into Treasury Bills, then the future disturbances in the market for long-term government securities have already been reduced to a minimum.

These conclusions are based partly upon information which the late Sir Austen Chamberlain elicited from his brother during the course of the debate in the House on the financial proposals of the Government and partly upon a study of what seems to have been the policy of the Treasury in the recent past. If they are anywhere near the truth, then it seems probable that the Treasury has decided to finance rearmament not by raising a new loan, but by devoting to the Defence Departments money which it has used in the recent past to purchase securities in the market. Instead of increasing the supply of government paper, it will simply reduce the demand of the National Debt Commissioners for it. A pre- W. T. Thornton[1] view of the workings of "supply and demand" would suggest the conclusion that the effect upon price of decreasing demand will be the same as that of increasing supply. To-day we are not entitled to take that view because we must include under demand "prospective demand" and under supply "prospective supply," and we now realise that the source from which changes come may affect anticipations in a different way, and these may cause an asymmetry between prospective demand and prospective supply. It is practically impossible on theoretical grounds to decide the probable influence upon the willingness of the public and financial institutions to continue to hold long-term government securities when, instead of anticipating a new loan on some occasion in the future, they must expect from time to time sales by the Treasury of bills and other securities in order to enable the Departments which hold invested funds to take up in their place Defence Department annuities. Assuming skilful management, which one is certainly entitled to do, it would appear that the effects of the infiltration method, upon which the Treasury seems to be going to rely, on the prices of government securities over a five-year period must be smaller than those which would result from the raising of a new loan along familiar lines. Moreover, as soon as the five-year period is up, the repayment of capital will begin and there will be at once a cumulative and increasing demand from the extra-budgetary funds for government securities as they re-invest the capital which has been paid off by the annuities.

It would appear, therefore, that everything that the

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[1] *On Labour: Wrongful Claims to Rightful Dues.* (1869) Chapter I. *passim.*

Treasury can do, short of direct currency manipulation, to maintain low money rates of interest, will continue to be done. It has adopted a policy which seems to have been designed to reduce to a minimum the disturbances to the market arising out of meeting rearmament expenditure by "borrowing." The total effects seem likely to be that while the Treasury has weakened its capacity to regulate the market for the next five years, it has, nevertheless, avoided the necessity for losing control altogether for a short period in the fairly near future by issuing a new loan. There can be little doubt that there will be from time to time during the five-year period new loans and further conversion operations, but these will be determined by the free choice of the Treasury and will occur at its own time and in its own way. In short periods it may have to increase the supply of Treasury bills to meet part of the defence requirements, but beyond this, rearmament finance seems to have been effectively insulated. The 3 per cent. interest rate charged to the Defence Departments does not necessarily give a guide to what the Treasury now supposes it may be necessary to pay to the market in the future. It was adopted, it appears, simply to give greater precision to inter-departmental book-keeping. Whatever opinions may be held as to the wisdom of the policy which has recently been embarked upon, it is impossible not to be impressed by the skill with which its technical details have been arranged. It was, perhaps, considerations of higher international policy and not of higher economics which caused the Government to be a bear of its own securities by the form in which it chose to announce to the world its five-year plan for defence expenditure.

Industrial Fluctuations

By Ralph Arakie

In this paper it is attempted to present, in the barest outlines, a theory of the trade cycle.[1]

Part I

1. The stocks of physical capital goods which exist at any moment of time are continuously undergoing processes of depletion and repletion. Their depletion provides society with consumers' goods and their continuous repletion allows of continuous consumption. The stocks are owned by capitalists, and their disposal is controlled by entrepreneurs. There are three important aspects of these stocks of capital: (*a*) their depletion and repletion take place through time; (*b*) the value of the capital stocks is in some way but not uniquely determined by the value of the consumers' goods which they go to form; and (*c*) the value of capital stocks is determined also by the rate of interest.

An economy is subject to uncertainty, and the presence of uncertainty implies that individuals are conscious that their actions may lead to costly mistakes. Mistakes are always the result of imperfect foresight; individuals are therefore concerned with minimising imperfection in foresight. This problem of minimisation is not simple, for obviously individuals cannot cease to act altogether, and equally obviously the realisation of imperfect foresight impedes certain actions which would take place were foresight considered perfect.

These circumstances give rise to speculation. All individuals conscious of existence through time are speculators; some individuals become Speculators[2] in a special sense, that is, they guarantee assured results to other individuals. The implication is not that those who have accepted assured results have not speculated in so doing, for they also have estimated the course of future events, but by establishing a group of Speculators, we have segregated the recipients of uncontractual income, changes in the size of which influence Speculators' decisions and the structure of the economy in a complex manner.

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[1] I attended a course of lectures delivered in 1934 at the London School of Economics by Dr. J. R. Hicks. The first four sections of Part I of the present paper have been much influenced by these lectures. On re-reading Mr. Keynes's *Treatise on Money*, Vols. 1 and 2, 1930, after Dr. Myrdal's "Der Gleichgewichtsbegriff als Instrument der geldtheoretischen Analyse" in *Beiträge zur Geldtheorie*, ed. Hayek, 1933, I was furnished with the immediate stimulus to the present paper. The influence of the analysis of the structure of production in Professor Hayek's *Prices and Production*, *1931*, will be recognised.

[2] We designate our specialised group by the use of a capital S.

2. The preliminary method proposed is the location of the Speculators at the consumers' goods end of the processes of production. The recipients of contractual income are the owners of the factors of production. Capitalists (who receive contractual income) advance wages, rents and interest (when paid before the sale of final goods) and Speculators refund capitalists money principle and interest thereon at the sale of the goods to the consuming public. The method requires that the factors of production enter long term contracts, *e.g.*, that all lending is debenture lending for specified periods at specific rates. These assumptions, which have little counterpart in actual business practice, will be dropped later on.

An individual is not a Speculator if he covers every future purchase by a future sale. He is not a Speculator if he purchases given quantities of goods at given prices for future delivery, buying future given factor services at given prices, and selling goods forward at prices which cover both the cost of the "raw product" and the added value. The logical position for the Speculator group is at the end of the processes of production, providing that (*a*) perfect future markets for all products are established, and (*b*) all cost items are known in advance through long term contracts with the factors of production.

The manner in which Speculators distribute the uncertainties which they assume in giving orders for future delivery of consumers' goods depends upon a number of circumstances all of which, except perhaps past experience of profit and loss, are mainly of "subjective" origin. There can be no question of objective measurement and consequently there is no fool-proof hedging or combination principle which can be brought into play. It is even impossible to determine whether Speculators as a group make an income from speculation, although uncertainty bearing may be (uncontractually) remunerated in the long run.[1] Two aspects of speculation stand out as essential data. (*a*) *Bona fide* Speculators must own capital assets. (*b*) Any estimation or combination of estimations must include an estimation of maximum possible loss, and the estimated maximum possible loss on future orders must not exceed capital assets. There is a third factor which has to be added to these two for the purpose of the present paper. (*c*) Present profits and losses on the sale of consumers' goods determine the estimations and offers of Speculators relatively to new future commitments. No absolute rule about the manner or even the direction in which estimations change with present profits or losses can be given, but we shall assume that present profits always lead to a lower estimated maximum loss, to anticipated future profits and hence to increased future prices. Present losses cause anticipated further losses unless future prices are reduced. This assumption contains much truth, even were it not confined to profits and losses on the sale of consumers' goods. With certain classes of goods the "turning point" in prices may come about because speculators, thinking that it may come, develop a bearishness. In the sale of final goods Speculators are more likely to wait until it does come through some cause other than their own anticipation. The reason for this is not far to seek. When, as in the real world, speculation is dissipated throughout the system, speculators are highly sensitive to the possible actions of their confrères of both their own and other markets. Our Speculators, on the other hand, can only be sensitive relatively to the consuming public; that the latter may suddenly begin to save more is considered a remote possibility, if indeed it is taken into consideration at all.

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[1] Vide Knight, *Risk, Uncertainty and Profit*, London School of Economics Reprint, page 362 ff.

3. The spot prices of the various final goods are given day by day in the consumers' market; if there are also markets for future deliveries of consumers' goods, and if there are markets for long term hiring of the factors of production, then the firms of the various stages of production will invariably show neither profit nor loss, providing that in the future markets each particular kind of consumers' good is bought forward for a period at least as long as the length of the period of production of the final good in question. Since speculation is confined to a group which is "located" at the end of the processes of production, production is carried on by firms under contractually paid managers who equate all costs with known future receipts.

If the process of production of a particular good involves through all its stages (firms arranged in temporal order of production) y years, then the final consumers' good will be bought forward for future delivery at least y years. In this case the firm at the beginning of the process will sell forward for a very short period and buy factor services spot. A firm in the middle of the process will sell forward $\frac{y}{2}$ years, and a firm at the end will buy and sell forward for y years. If, on the other hand, the final good is bought $2y$ years forward, the period of production remaining y years, then the firm at the beginning of the process will contract for the sale of output y years in advance and buy factor services for that period also. It is unlikely that future orders will cover a period longer than the process of production of a particular good, for the period forward for which a final good is ordered sets the limit to the "economic horizon"[1] of that good. For each particular final good there is an economic horizon depending upon the length for which that good is quoted forward. The period of production of a particular good is determined by the economic horizon for that good, which in turn is determined by the technical productivity of capital and the degree of uncertainty attached to forward purchases.[2] The absolute distances of the various horizons are unimportant for the purposes of this paper; only changes in the horizons as caused by changes in Speculators' estimates will be discussed. Since we may assume the technical productivity of capital to be known in advance, the degree of uncertainty is the major variable involved.

4. By "general profits" is meant not only that the aggregate of Speculators' money gains at the time of sale to final consumers is positive; when Speculators make general profits (losses) the sale of *most types* of final goods is attended by profits (losses). The reason for this definition will become apparent as the argument develops. For the moment we start from a position of disequilibrium (characterised by general profits, positive or negative, as defined above) and examine the change to another position

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[1] Tinbergen, "The Notions of Horizon and Expectancy in Dynamic Economics," *Econometrica*, July, 1933.

[2] The degree of uncertainty attached to forward purchases of "novelty goods" is very great, and methods employing a relatively large proportion of labour on short contract predominate in this field of enterprise.

(also of disequilibrium). We thus observe the general horizon as it recedes or approaches.

When Speculators, who in a continuous system are daily buying forward and daily selling spot, make profits or losses, they give rise to a new situation by changing (*a*) the quotation of future prices and (*b*) the date of future deliveries. The case of general losses is more interesting than the case of general profits, since, as we shall show, changes in the rate of discount may be relatively ineffective in the case of general losses. In other respects the description of the effects of general losses only requires a change of sign to become the description of the effects of general profits. If Speculators are making general losses, both of two effects are likely to be exhibited. The new contracts drawn up for future delivery of final goods will be fixed at prices lower than those prevailing, and the future market for the more extensive delivery dates will shrink. It is impossible to determine *a priori* whether the first effect of general losses exerts itself more in one direction than the other from the point of view of reducing future prices (a certain result) and reducing the time extent of forward delivery dates. The reduction in forward delivery dates may in some cases present difficulties due to technical fixities in the time structure of the economy, or the absence of " close " technical possibilities. There seems some ground for believing that the framework of the economy may and does remain relatively unchanged in partial employment, at least for considerable periods. Moreover, the partial employment of, *e.g.*, labour in the same time structure would appear to be the more likely where, as in actual practice, speculation is widely dispersed throughout the system. In any case, under our assumptions, a reduction in the time extent of future deliveries results in an unequivocal decumulation of physical capital, since by definition managers of high stage firms unable to assume uncertainty cannot restock as depletion (necessitated by contractual delivery) takes place. The lower future prices offered will lead to decumulation of physical capital depending upon the elasticities of supply of land, labour and capital, *i.e.*, with cost reactions. Complicated as a detailed analysis of cost reactions would undoubtedly be, the general position is easy to discern : the total money income of society (through total costs which are determined by the offers of Speculators) must decrease. For the present, the only question which we have to ask ourselves is whether the decrease in costs can be effected without a reduction in physical capital.

The *first* appearance of general losses on the sale of consumers' goods will not necessarily lead to decumulation of physical capital, for if the owners of land and labour factors are willing to provide the same quantity of services for a lower remuneration per unit of service, the whole of the necessary fall in costs will be brought about even if the discount rate is unchanged. To regard both the original factors as rent supplied in the short run is perhaps the most appropriate single assumption, and certainly the most favourable for the support of the view that a new equilibrium is brought into being by reductions in costs. There is no initial decumulation of physical capital providing only that Speculators concentrate solely upon reducing future prices, and do not attempt to reduce the time extent of future orders. Nevertheless, there is a fall in the total value of capital unless there is an appropriate fall in the discount rate, and by concentrating upon changes in the value of capital, we shall see that while under favourable assumptions initial general losses do not necessarily lead to physical decumulation in the *first* instance, whether there is or is not *initial* decumulation is of little import to further developments in the economy.

5. A fall in the total value of capital, the rate of discount remaining unchanged, is the result of losses, actual and/or expected, by Speculators on the spot sale of consumers' goods. The advantage of presenting the speculative function in the manner outlined is that *all profits and losses appear as capital gains and losses.* Since it is impossible for the owners of capital other than Speculators to make losses or profits, whether these are considered in income or capital value terms, any unexpected change in the total value of capital must involve changes in the value of Speculators' (money) assets. The balance sheet of a Speculator will be drawn up thus :—

Assets.	*Liabilities.*
(1) Bank Balance.	
(2) Estimated sale receipts on future deliveries, discounted at a rate, i_r.	(3) Total of future commitments (known future purchases), discounted at the bank rate, i_b.

When, at the moment of closing a deal, the balance sheet is drawn up, the items (2) and (3) are equal. But estimated sale receipts in (2) cannot be equal to known future commitments in (3), for i_r must be greater than i_b. It is not only the equality between the items (2) and (3) which is relevant to Speculators. This equality could be obtained in numerous ways. The constituents of item (2) are important. Estimated sale value in (2) must be derived solely from considerations of the market and cannot be fictitiously construed for the purpose of equating (2) and (3). And similarly i_r is a true estimation and its size is determined by adding a risk coefficient to the known prevailing rate of bank discount, i_b. It will be noted therefore that the amount which a Speculator is willing to bid for a future purchase depends upon the value of item (2), which in turn is derived from two estimations.

If after closing a deal a Speculator becomes pessimistic and has to modify his estimation of sale receipts (due perhaps to a fall in spot prices), the present value of net assets $\Big((1) + (2) - (3)\Big)$ is reduced if i_r and i_b remain as before. The loss is made good from item (1). The pessimism may not be confined to the estimated value of sale receipts; it may extend its influence in increasing i_r, due to less confidence in estimating risks. Thus, even though i_b is reduced, i_r may increase, the reduction of the element i_b in i_r not being sufficient to offset the increase in i_r, due to the increase in the risk element. It is this circumstance which accounts in large part for the "ineffectiveness" of changing discount rates, to as great a degree perhaps as the extent of the fall in estimated receipts. Even neglecting the possibility of i_r and i_b moving in opposite directions, and assuming that a reduction in the bank rate of discount i_b brings about a corresponding fall in i_r, net assets are still lower as far as items (2) and (3) are concerned, for the present value of liabilities increases *pari passu* with the present value of assets in (2). It is the increase in item (1) which may offset the capital loss. If we remember our assumptions of long term contracting and apply them to deposits in banks, or if for a bank deposit we substitute fixed interest-bearing securities in (1), then net assets may be kept intact following a fall in the estimated value of receipts and a reduction in i_b. The maintenance of the value of capital after an initial fall in estimated future receipts necessitates the following conditions: i_b must be reduced (and remain positive), the reduction in i_b must be passed on and lead to a reduction in i_r, the fall in the estimated value of future receipts must not be too severe, and item (1) must be sufficiently large. There will be various combinations of these conditions, and if the particular combination in any situation is propitious, the first appearance of general losses may be counteracted.

A lower discount rate which follows closely upon a pessimistic turn in estimations of future receipts, offsetting either partly or wholly capital losses, will counteract to some extent the tendency of Speculators to avoid new future commitments on the former scale. It can never *eo ipso* lead to bullishness in estimations of the future price level of final goods, but it may check the growth of pessimism. For, firstly, the necessary cover for *bona fide* Speculation is not reduced as much as it would be were there no fall in the discount rate. Secondly, a lower rate of return on bank deposits will tend to lead to a redistribution of assets in favour of taking more extensive risks.

6. We may assume a system working in quasi-equilibrium until totally unexpected losses on the sale of consumers' goods occur, due, let us say, to capricious hoarding on the part of the recipients of current money income. These unexpected losses must lead to further expected losses on existing unliquidated contracts for future delivery, losses which we will assume are not offset by a reduction in the rate of discount. We can now further assume that the time extent of renewed future contracts is not reduced, but that only the prices for future delivery of these renewed contracts are reduced. Is a new quasi-equilibrium established? Will the new revised contracts prove to have been justified in terms of future results?

At the heart of the cumulative process are three primary relationships:

(*a*) Current physical investment output is destined for future consumption.

(*b*) The price level of the present output of consumers' goods is decisive in determining the volume of money costs expended in the production of currently produced investment goods.

(*c*) Payments for the services utilised in the production of currently produced investment goods buy the present output of consumers' goods.

After general losses (due, let us say, to capricious hoarding) Speculators will only renew contracts at lower prices and the factors of production will receive a diminished total money income. Speculators are prepared to take losses on all unliquidated contracts, losses equal to the initial fall in prices which occurred as a result of the new hoarding. They expect to avoid losses on the renewed adjusted contracts, for these have been drawn up at prices at least somewhat *lower* than current prices, due to the reduction of their money assets (cover against insolvency) through current and expected losses on past contracts. There is no reason present why Speculators should *expect* to suffer *increased* losses on unliquidated contracts, *i.e.*, losses greater than those given by the initial fall in prices. *Yet this in fact will occur.* The cost adjustments following the initial losses create further losses totally unexpected, for the income of society (which buys the present output of final goods coming forward on old contracts) has been reduced below that amount which was formerly spent upon the purchase of consumers' goods at the time that initial losses emerged. In other words, the income of society has been reduced below the old equilibrium total income minus the original capricious hoarding, so that even though all new hoarding ceases, the losses on unliquidated contracts will exceed expectations. These unexpected losses lead Speculators to believe that they have not been sufficiently pessimistic and the further renewal of contracts (contracts are continuously renewed) will only take place with a still further reduction in cost items (factors' income), an attempted adjustment which destroys the terms upon which it is made.

Had Speculators been willing to take the unexpected increase in losses on those contracts which had not terminated, without reducing any further future prices (and consequently income through current cost items) the initial adjustment would have left the situation ready for a new equilibrium. This sort of action can, of course, never take place, for the increased unexpected losses on past contracts which are at the point of being liquidated are not viewed by individuals as a necessary consequence of the first adjustment. (Had dis-hoarding taken place at the appropriate moment and to an extent which, while leaving losses on past contracts, did not increase them, the cumulative process would not have set in.)

The foregoing analysis is based upon three primary relationships and the assumption of an unchanged bank rate of discount. It demonstrates that a cumulative process has an inherent momentum of its own and that there is no tendency present for forces automatically to come into play and establish a new equilibrium. We must now introduce a variable discount rate and examine whether partial or complete offsetting of capital losses prevents the initiation of cumulative disequilibrium.

The crucial point of this analysis of the cumulative process is the result that the value of new future commitments falls short of the value of old contracts by more than the initial fall in spot prices. This result was derived directly in the first instance from the presence of a smaller cover for estimated maximum losses. Now, if by reduction of i_b and a corresponding reduction in i_r, capital assets are kept intact, this cover is not reduced. But the cover-against-insolvency aspect of assets is not the only determinant of the willingness to take on new commitments. Speculators may be content to hold government securities of enhanced value without renewing commitments on the same scale, and anticipatory factors such as those discussed in the *Treatise on Money*[1] may very well lead to a smaller volume of future orders at prices below those prevailing. Once the cumulative process has set in further reductions in i_b become ineffective and the physical capital of the community is not maintained, either the general horizon drawing in or the structure of production working at partial capacity.

Cumulative disequilibrium is not engendered by a " time lag " but by a peculiar constellation of decisions effective through time, and by the simultaneity of the occurrence of moments which lose their identity and consequently their specific functions because they *do not* begin to act at different time points. Cost reductions and further losses occur together ; were cost reductions not to induce further losses and *vice versa*, the cumulative process would not easily find its initial momentum.

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[1] Op. cit., Vol. I, p. 250 ff.

Part II

1. A process of saving and investment which takes place without initiating cumulative disequilibrium is one which does not result in either general losses or general profits to Speculators as a class. In describing how saving and investment can proceed in a quasi-equilibrium manner we cannot fail to become aware of the nicety of the adjustments required—a nicety certainly absent from a more haphazard economy than that under discussion. There are, however, compensating advantages in an economy not so rigidly subservient to Speculators. These will be discussed in Part III of this paper.

We must be wary of approaching the problem from the wrong end, one of the errors of most of the work on trade cycle theory in the past. That wrong end is the Monetary Policy approach. The prices which the Speculators are prepared to guarantee for future final goods are a matter of their *ex ante* accounts, and while it is undeniable that the idea of the success of a given monetary policy which authority may announce may influence the drawing up of these accounts, it is nevertheless true that starting from general quasi-equilibrium it is very unlikely that even a great deal of "public" feeling will translate itself into new contracts for a greater volume of final output for a longer time distant at the same prices per unit of output. It is essential to realise that our procedure is not first the advocacy of a given policy and then an adequate dovetailing of reactions to fit the policy. The essential proposition, which applies only to an initial position of general quasi-equilibrium, is connected with the *ex ante* accounts of Speculators relative to the prices of final goods which they contract to buy in the more distant future and the sale price of these goods when they come into the consumers' market in that more distant future. If the quasi-equilibrium is to be continuous, the final sale receipts of the increased volume of consumers' goods must be just sufficient to cover all past costs, *i.e.*, refund capital and interest. It follows therefore that if the *ex ante* accounts were drawn up for lower future prices, the relevant *ex post* account which will be drawn up when the final goods are sold must not prove the past *ex ante* accounts to have been erroneous either one way or the other[1]; the relevant price level for goods sold now must be lower than formerly if lower prices were expected when the future contracts were drawn up.

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[1] *Vide* Myrdal, op. cit., for the distinction between *ex ante* and *ex post* accounts.

2. Speculators, who in the renewal of future contracts at any given moment, contract to buy larger final outputs at future dates still more distant, will only be able to offer lower prices because as *bona fide* Speculators their maximum possible losses would have the same provision in the form of capital assets as formerly, while these maximum possible losses would increase with (*a*) the greater volume of future purchases and (*b*) the greater uncertainty of more distant dates. Alternatively, if we consider that the capital at the disposal of Speculators increases gradually with the increase in social capital, total liabilities would increase at a greater rate than Speculators' assets, were future prices not to be reduced. Thus, even though Speculators " see no reason " why prices should fall, they will nevertheless offer less per unit of future order. They may hope for the same prices in the future as those at which they are now selling, but they do not act on this hope. Continuous equilibrium requires equilibrium rewards for actions undertaken, not for hopes entertained.

3. The question now arises : will future contracts ever be drawn up for larger outputs and more distant dates at lower money prices per unit ? We assume the technical productivity of capital to be known and certain. The greater physical productivity of more capitalistic methods combined with lower interest charges enable under some circumstances managers (of integrated processes) to fall in line with Speculators' offers. In the next section we characterise the nature of these circumstances. For the moment it is necessary to remark that the contracts between managers and Speculators are not necessarily equilibrium contracts in the sense that the future results given by the sale proceeds of final goods to consumers will just cover costs ; this is something which can only be known in the future. But they must be equilibrium contracts in the sense that the future market for labour, capital and land is one in which no exchanges are completed until those future prices are discovered which just meet the total of cost items and equal the *guaranteed* total repayment of capital and interest on the goods which are to emerge on the consumers' market. It need not be said that this sort of Walrasian market is established in the real world, albeit

very imperfectly; nevertheless the recognition of its existence, however imperfect, is of importance to our argument. Under our assumption that only Speculators at the end of processes make profits or losses, it means that, however low the rate of discount falls, the demand for capital in money form will never be so great as to raise factor prices beyond that point where, taking into account increased physical productivity, average costs equal Speculators' offers for future output. This result can be expressed in another way: the *price* discounting for final goods, increasing as it does for contracts relative to more distant outputs, will offset to some extent, and perhaps very rapidly, lower discount rates. Value productivity must take uncertainty into account[1]; the net return on capital is an unknown quantum, the estimation of which will determine the demand for capital. In the present paper, the Speculators make these estimations, and these estimations relative to a sufficiently distant future output are so low (or in other words the risk coefficient is so high) that the demand for money loans at a zero rate of interest is *limited*, the known increased physical productivity translated into value productivity by prices which Speculators offer being insufficient to refund any increased cost items (determined in the Walrasian manner).[2] The problem of the stability of the social economy rests upon whether the discount at which future prices are quoted is simply a function of time (period of production) or whether it is a function of *increases in an already established* period of production. It would appear to involve both aspects, for the above argument is only strictly applicable to a large discontinuous fall in interest charges.[3]

4. We now turn to the question of the change in the size of the total money income of the community which follows a reduction in the rate of discount. The Speculators' demand schedule for future deliveries of final output depends upon the price discounting (risk) associated with the various forward purchases. We have already suggested that this

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[1] Myrdal, op. cit., p. 385, sect. 20.

[2] Walras, *Eléments d'Economie Politique*, 1896, 21e leçon. "Tâtonnement en vue de l'égalité du prix de vente et du prix de revient des produits."

[3] Speculators as a class make no capital gains or losses in quasi-equilibrium, although they may make an income equal to wages in an alternative employment. Were Speculators not to take more extensive risks following a small fall in the rate of discount, an average rate of profit equal to the extent of the fall in the discount rate would accrue to them, and competition would eliminate this.

price discounting grows at an increasing rate the further forward deliveries are arranged, that it is an increasing function of increases in an already established period of production. Now if the greater final output per unit of time made available by longer future contracts does not increase at an increasing rate the further contracts (periods of production) are extended in the future, it follows that any large extension in forward buying can only be made at prices which reduce the total money income of society and the total value of physical capital. It does not follow, however, that a *small* extension in future delivery dates reduces the total value of Speculators' offers per unit of time. For it may only require a (relatively) small fall in future prices to induce Speculators to place orders for a more than proportional increase in the volume of goods. A small extension in delivery dates allows of an increase in output more than proportional to the fall in future prices. It seems reasonable to assume this kind of elasticity of future demand *in equilibrium*, for its absence would make progress difficult, if not impossible. Were it not to hold, both piece and hourly wages would fall, whereas assuming this kind of elasticity, hourly wages for the same type of physical labour must increase with increases in the social capital. We conclude, therefore, that in dynamic equilibrium any extension in the average period of production induced by a fall in the rate of discount is accompanied by an increase in the money income of the community.[1]

5. In a community in which the quantity of capital increases continuously and evenly, the output of final goods also increases without interruption. Saving cannot be recognised in any actual diminution in consumption, it is discovered analytically in the proposition that final output would be greater than it is at any moment of time were no net increases in stocks taking place. When increases in the stock of capital are jumpy or discontinuous, then the volume of final output is actually diminished for a period. Let us first examine the case of a sudden increase in the amount of capital stocks. In this case, the construction of new investment goods, as distinct from simple reinvestment, necessitates a temporarily diminished output of final goods. Were all capital goods of a non-specific nature,

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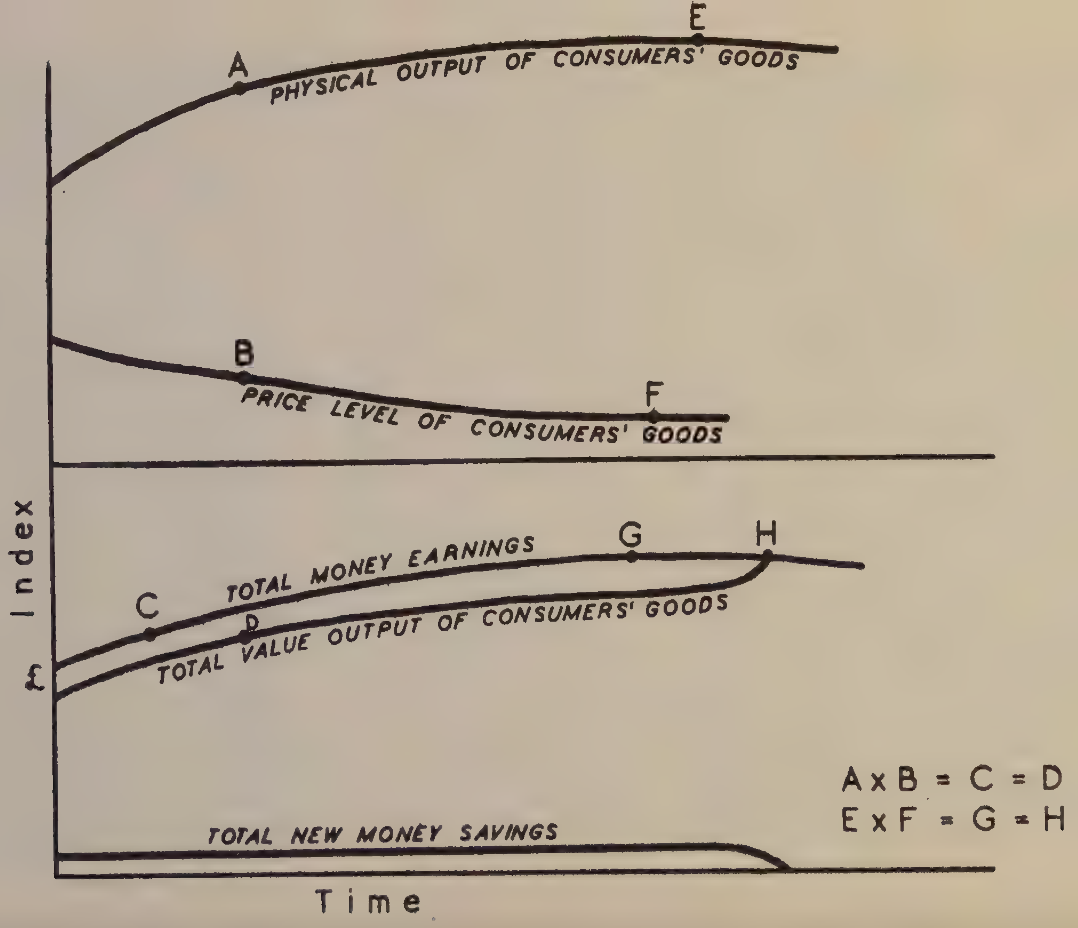
[1] At a later stage of the argument it will be seen that unless the elasticity of demand for future orders is greater than unity, savings must always result in disequilibrium.


A
E
PHYSICAL OUTPUT OF CONSUMERS' GOODS
B
F
PRICE LEVEL OF CONSUMERS' GOODS
Index
£
G
H
TOTAL MONEY EARNINGS
C
D
TOTAL VALUE OUTPUT OF CONSUMERS' GOODS
TOTAL NEW MONEY SAVINGS
A x B = C = D
E x F = G = H
Time

and were no future contracts outstanding, this temporary diminution could take place as soon as new investment is commenced. We can neglect at this stage the question of the specificity of capital goods and concentrate upon the time interval between new investment and the diminution in final output from the point of view of renewal and non-renewal of contracts for future delivery of final goods. Now while new investment can always be commenced without delay, since contracts are continuously lapsing, decisions not to renew old future contracts in the same volume, while releasing factors for the building up of net additions to stock, *nevertheless leave a stream of final goods to emerge in the future on those contracts which have not yet run their term.*

Our criterion for continuous quasi-equilibrium is the absence of either general gains or general losses to Speculators. There are three distinct stages of adjustment necessary to conform to this criterion during a change in the volume of capital. We assume throughout that the change is induced by a lower rate of discount, and that the particular combination of Speculators' estimations and known physical productivity leads to a greater total money income of society.

(1) *First stage.* If no losses or profits are to emerge during the whole transition, the same volume of income as formerly must be spent upon the output of final goods until the volume of this output is diminished; *i.e.*, there must be new saving to an amount equal to the increase of total income.

(2) *Second stage.* During the stage when the output of final goods is diminished, some saving, in the sense of not spending the whole of the increase in money income which follows upon the new investment, will be necessary if profits are not to appear. The amount necessary may be greater or less than before, depending upon the prices which Speculators offer for the diminished output.

(3) *Third stage.* The total increased income must be spent upon the sale of final goods when these latter come forward in their permanently increased volume. If this is not the case, total cost items will not be refunded in Speculators' receipts.

The emergence of profits or losses at any stage implies the possibility of cumulative disequilibrium, for even though the volume of final output for periods quite distant is already determined, the terms on which contracts are renewed depend upon present gains or losses.

The delicate adjustments required, especially during the second stage, lead one to believe that equilibrium progress could at the most only be brought about by a concatenation of accidents. Those factors which determine Speculators' offers for the diminished output on the one hand, and consumers' outlay on the other hand, seem independent to a high degree. Even the task of discovering the factors determining Speculators' offers for the diminished output is not easy.[1] In their contracts with managers (of integrated processes) Speculators come to terms for the reduced output and the later larger output not independently, for the factors of production receive remuneration according to their marginal physical products discounted for the various periods at a uniform rate.[2]

With continuous and even increases in the volume of capital the second stage of adjustment disappears, and we are left with an adjustment necessitated by the following circumstances. The money prices of final goods are continuously falling, at each moment of time Speculators are just covering commitments and are only willing to take further greater commitments at reduced prices. Since the total value of output is nevertheless increasing, the money income of society is also increasing. But at any moment of time the total money income received is always greater than the total of cost items undergone to produce the final output of that moment. Thus the money income of society is always a step in advance of the total equilibrium value of final output. Saving is necessary.[3]

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[1] Keynes, op. cit., Vol. I. *Some Special Aspects of the Bank Rate*, 1. This is a very important paragraph in the *Treatise*.

[2] At a uniform rate, because loans to managers are riskless if Speculators hold sufficient cover. Entrepreneurs bear the uncertainty and the rates at which they discount assets include estimated risk coefficients which may vary widely for similar orders in perfectly competitive conditions, since there are many personal equilibria distributions of assets in quasi-equilibrium. The discounting of Speculators' *liabilities* would involve no uncertainty or risk coefficient were the bank rate of discount guaranteed constant for sufficiently long periods.

[3] Certain important complications are neglected in this paper. Thus when it is said that the earnings of the community are a step in advance of the equilibrium value of the output of consumers' goods because the former quantity is represented by the value of current investment, it must not be forgotten that investment is spread over time and that future orders for larger outputs of greater value do not result in an *immediate* increase in the income of the community *equal* to the increase in the value of future orders. But there is *an immediate* increase in the community's income, and when continuous new investment is considered it is legitimate for certain purposes to think in terms of the whole increase taking place immediately. There are other related complications which cannot be dealt with in this paper.

We have approached the problem from the unusual angle of inquiring how much new saving is required to permit a given increase in investment under equilibrium conditions. We have assumed a given fall in the discount rate, that fall stimulating the demand for loans on the basis of estimations of future prices and the volume of final output (assumed known) which will come forward on the new longer contracts. Saving was unrealistically regarded as a dependent variable. We now turn the question about and ask: how much investment must take place in order that a given amount of new saving should not cause disequilibrium? Although the answer to this question follows directly from the previous analysis it is necessary to emphasise that an expected future price level may automatically come about, providing that either (*a*) new savings are continuous and even or (*b*) changes in the volume of new savings are correctly anticipated. In other words, provided the volume of new investment is correctly regulated by changes in the rate of discount, no profits or losses on the sale of consumers' goods will appear.

Losses on the sale of consumers' goods must appear in an economy once the condition of stationariness is changed by new money saving unless the money income of the community is increased by an amount equal to the new savings.[1] For there are contracts for the future delivery of consumers' goods outstanding. The first adjustment, therefore, must be one on the size of the total money income of the community. The money income of the community represents payments for the production of investment goods, *i.e.*, future consumers' goods, and any adjustment of the volume of these payments can only come about by an adjustment of the volume of future orders. For a given amount of new saving Speculators must be induced to give orders of a value greater than preceding orders by an amount equal to the new saving. They are so induced by a fall in the discount rate which shifts them on to a new combination of volume of delivery and expected prices, a combination which gives a greater value to outstanding contracts, and hence a greater current money income to society.

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[1] If the community is doing a given amount of saving measured as a proportion of the total money income, and if there is to be no change during the period under review in the money demand for consumers' goods, then the money income of the community must increase so that a new and possibly greater proportion of the increased total money income can be saved without diminishing the money demand for consumers' goods. A determination on the part of the public to save a certain amount of *real* income, measured as a proportion of total real income, can always be offset by monetary adjustment, since each individual will imagine himself wealthier if without spending his whole increased money income he is consuming as much as before. If the reaction time of the adjustments both of savers and monetary authorities is not instantaneous complications ensue, for savers, and hence the central bank are likely to have to make more than one adjustment for a given amount of "real" saving. There will also be an accidental redistribution of real income in so far as some individuals do no saving. It would seem that there is room for a distinction between "real saving" and "imagined real saving." The latter is the only actively relevant factor.

There are independent reasons why the expected future price level should be lower than the prevailing spot price level. These have already been discussed. What grounds are there for believing that an expected lower price level will in fact come about? When the larger output of consumers' goods comes forward the income of the community will equal the cost of production of this output, providing that the end of new savings has been anticipated and the increase in the income of the community has been checked by stopping new investment through a refusal further to reduce the rate of discount. But if the new saving continues, the anticipated lower price level will also prove a reality, providing that this new saving is offset by an increase in the value of new future orders. Thus we can see that continuous new saving is not inconsistent with the criterion of zero profits *and* a falling price level. As the total money income of the community increases, so also does the total cost of production of final output; the former quantity is a step in advance of the latter, but this is necessary in order to offset saving so that the actual total expenditure on consumers' goods equals the total cost of their production, while at the same time the capital of the community increases.

It may be helpful, in order to grasp the relationships involved, to imagine savers grouped together in a well informed association. They decide in concert to do no more new saving, but, keenly aware that the whole of current income is composed not only of payments in respect of current final output but also in respect of a greater value output of the future, they give notice of their intentions for fear of raising the price level of final goods above its equilibrium station. An equally well informed central bank, on hearing of this decision, holds the discount rate *in status quo*. Our savers now put their decision into effect *gradually*, increasing their expenditure as the increase in final goods becomes evident. At the point where they begin to spend the whole of current income, the economy is in stationary equilibrium. The lower future prices of the past are borne out in lower spot prices. For how could it be otherwise, the total income of the community being paid on the basis of a lower price level for final goods ?

To return to the case of initial new savings, it is conceivable that under special circumstances the total spent income of the community may decrease without causing disruption. When intermediate products, and particularly what, in Mr. Keynes's analysis, are called " liquid consumers' goods " are of a non-specific nature, *i.e.*, goods which have multiple transformation possibilities, *e.g.*, coal, losses on the sale of consumers' goods co-existing with capital gains in high stages (due to the simultaneity of a reduction in the discount rate and new saving), may result in the resale of future purchases to managers in higher stages. The volume of resale and consequent transformation of goods from low to high stages may prevent a fall in the price level of final goods, a fall which would otherwise take place were total expenditure to decrease. In a realistic analysis this argument must be dismissed, for liquid consumers' goods are predominantly of a highly specific nature (not to mention specific durable instruments) and transformation is impossible.

Part III

1. To regard the economic system as a perfectly co-ordinated chain of future markets is doubtless an abstraction of the pattern of the real world, if it is not actually the construction of an imaginary world. But in many important points this construction has parallels in the real world, and the existence of future markets (*e.g.*, organised primary product markets) is one of the least important of these ; for the total absence of organised markets would not involve discarding the construction. The flow of intermediate products is a movement in response to the demand for final goods. It cannot be maintained that prices are " telegraphed " throughout the system, still less that cost items are determined in advance through long term contract with the factors of production, but equally it cannot be denied that the prices of intermediate products are related to the expected prices of final goods and that enterprises are conducted with a view, not only to spot factor prices, but also to future factor prices. The speculative function is not confined to a specialised group dealing in

finished articles; it is spread throughout the system, conducted by numerous groups. But however numerous the groups, and however short the section of the period of production between groups, speculation is never absent from enterprise.

2. Much business in the modern world is conducted by manufacturers who produce both on order and independently of contract and who buy in similar manner raw products both for future delivery and spot. When a high stage firm accepts a contract for future delivery, it does not necessarily follow that a manufacturer in a low stage process has made a correspondingly long future purchase, for dotted about the system there may be speculators who either buy future and sell spot, or who buy further forward than they sell. These speculators (*e.g.*, wholesalers) correspond to the Speculators of this paper. When the speculative chain is broken up in this manner, the prices of final goods influence the volume of investment of the economy as a whole, less directly and less rapidly than they would were future markets to extend upwards throughout the whole structure from the lowest stages. Thus losses or profits on the sale of consumers' goods may be found with profits or losses on the sale of high stage intermediate products. Any complete analysis must take into consideration a true time lag of this kind, a lag which provides a certain armour against what would otherwise prove to be the devastating effects of *small* changes in the volume of money expenditure upon the purchase of consumers' goods. While continuous and prolonged losses on the sale of final goods must infect the economy as a whole, increases in the volume of investment, due either to inventions or an isolated optimism in high stages, will offset losses on the sale of consumers' goods, losses which would have led to cumulative disequilibrium were speculation more closely knit together. It would be false, however, to represent the economic system as endowed with a very sluggish nervous system. The sales of consumers' goods are regarded as indicative of the prospects of investment in high stages, *e.g.*, motor car sales and rubber, and even excluding speculation independent of production there is no reason to believe that entrepreneurs are absolutely insensitive to the statistical data published in trade journals.

But whether directly sensitive to distant events or not, entrepreneurs become indirectly informed of the trend of spot prices of consumers' goods in the prices of their own (high stage) turnover. The absence of organised future markets in an economic system conducted by entrepreneur capitalists implies that each section of a period of production is a future market of given extent enclosed by the firm, and when vertical transactions between adjacent firms are conducted by spot dealings only, the entrepreneur capitalist, in so far as he does not search for information relative to a distant future, is provided with two objective criteria upon which to base his estimations of the prospects of investment—the trend of spot prices and the current rate of discount. It is true that any investment in durable capital must be influenced by ideas of quite distant future prices and long term rates of discount, and these necessary attempts at lengthy foresight must not be considered unimportant. But the very fact that they are so lengthy makes it rather a hit-and-miss business. It is not surprising therefore that current trends of spot prices and discount rates should prove more weighty in the minds of entrepreneurs. The trend of spot prices is usually the more important indication, at least from the point of view of reinvestment in working capital.

It is not difficult to trace rising or falling spot prices as they work their way through the system, and their effect upon the volume of replacement of physical capital. The firm purchases raw products and sells its particular articles almost continuously throughout the year. The entrepreneur capitalist, in his attempts to maintain or increase his wealth, is activated by the same principles as those which influence a Speculator. His assets consist of a certain value of capital goods, given by discounting his future sales at a rate i_r, taking into account certain estimated future costs in the form of wages and interest.[1] Unexpected losses on spot sales lead to further expected losses, and to a refusal either to take the same volume of goods from a previous stage or only to take them at prices which are low enough to fit in with an increased i_r. The volume and the value of spot sales throughout the system will contract, some entrepreneurs holding off from selling in the case of non-perishable goods, and all entrepreneurs refusing to buy in normal quantities. This is in fact an attempt to transfer assets from instruments to bank deposits, the success of some frustrating that of others. It is evident that if new saving is not counteracted by an increase in total income, losses must work upwards from retailers to firms in the higher stages. The speed of dissemination of losses or profits is a function of the continuity of sales throughout the system, and what has been aptly termed the "sensitivity" of entrepreneurs. This sensitivity must always have been very great, but the modern organisation of business has given it extended rein, so easy is it to transfer assets from one category to another. Of the entrepreneur's reaction to sticky factor prices in his estimations of capital value as affected by future purchases of services, it is undoubtedly true to say that wage fixing must tend to check any tendency to reinvest during a depression. Since it is very unlikely that the money income of the community is even partially maintained by trade union action and general rigidities, it cannot be argued that entrepreneurs' hesitancy, due to sticky factor prices, is offset from the point of view of the general situation by causes tending to maintain the price level of consumers' goods.

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[1] *Vide* Myrdal, op. cit., p. 391ff, for an admirable exposition of the entrepreneur's estimations and capital values.

PART IV

The correct management of the central bank's discount policy involves foresight and anticipatory action, the effectiveness of discount rate manipulation being confined in the case of general uninvestment to the point of initial losses, and only then when the latter are small enough to be offset. Small initial losses on the sale of consumers' goods may cause, immediately, large derived losses on the sale of intermediate products of previous stages, since purchasers (firms) are bound to exhibit some hesitation. If, therefore, new saving is not anticipated, its first appearance may involve general losses too great to be offset by even a quick and steep downward movement of the discount rate. On the other hand, discount policy can always be made effective in checking investment, for theoretically and practically there is no upper limit to charges for loans, and in the case of the incipient boom the role which correct anticipation will play does not present such difficulties. Whereas a fall in the price level of consumers' goods is not a sign that losses are being suffered, a rise in this price level is sure to be accompanied by general gains. General gains may be present even though the price level of consumers' goods has a downward tendency, and in this case also the price level of consumers' goods provides a poor index upon which to base policy.

Once a boom is well on the way there seems no method by which the discount rate can be raised by an amount sufficient to "stabilise" the existing situation. The boom is characterised by what may be termed a "general real disproportionality" and may be described in the following manner: those methods of production already in use or planned are overstocked relatively to their optimal *technical* working. Large stocks which have been built up as speculative holdings must be physically liquidated by being used up. As soon as it is realised that technically optimal stocks have been exceeded, the estimated value of existing stocks must fall, and capital losses are inevitable. The recognition that speculative holdings of stocks are excessive is likely only to follow a rise in the discount rate, but once the position dawns upon the entrepreneur community, the extent of this rise is of relatively little import. There is no reason inherent in the economic system, theoretically considered, for believing that a depression cannot occur without preceding over-expansion. Nor are there grounds for believing that the process of liquidation of stocks takes place during a depression up to that point where technically optimal quantities are reached. In a system subject to uncertainty, stocks are *never* technically optimal, a fact which deprives this concept, definite enough in stationary analysis, of exact meaning.

The problem which presents itself to central banks, ideally considered, and apart from international complications, concerns judgment of the following kind: at what point to enter upon a positive policy of preventing a further fall in the price level of consumers' goods. This is a two-fold problem, involving on the one hand some sort of estimation of technically optimal stocks, considered not only in terms of the requirements of present methods of production, but also with a view to the possible increased requirements of "legitimate" new investment in the early future; and on the other hand, the *modus operandi* of price level manipulation. While the problem of estimating stocks is exceedingly difficult, it is not absolutely impracticable.

Something could be found out about the stocks of primary products, and this could provide a rough index for stocks in general; for crude as such an index would be, it would nevertheless give some objective criterion for judgment.

Participation of the joint stock banks in the market for industrial securities implies that the latter would be under direct control from the point of view of price only if the central bank is also a participant. Were the central bank to control security prices in this manner, they would have at hand a method of checking uninvestment. There are, however, certain difficulties to be overcome. In the first place, it is not certain that the artificial maintenance of the price level of industrial securities will ensure that the quota of reinvestment will be prevented from falling; for if the bank comes on to the market after the price level of consumers' goods has fallen below its equilibrium level, the quota of reinvestment in working capital will nevertheless diminish. Secondly, if the banks are in the market for both government bonds and industrials, the price level of each of these categories must bear some relation to relative net earnings, and the more intensive desire to hold bonds will not be easy to check. Thus it is difficult to judge the measure of success which a policy of open market dealings in common stock is likely to achieve. It may give more confidence to the promoters of new issues, but it may also inspire excessive optimism. The great advantage to be derived from central bank participation in the market for ordinary shares lies in the strong palliative action in preventing *financial* booms and crashes.[1] It seems to the present writer, that in periods of abnormal trade the markets in securities and primary commodities tend to run amok independently of *entrepreneurs*' speculative decisions, dragging nevertheless entrepreneurs with them, much to the injury of the economy.

The certain check to excessive uninvestment will be found in the ability to prevent a further fall in the price level of consumers' goods by direct action. This sphere of activity lies outside the ægis of the central bank and requires government intervention. This could take the form of pure consumption inflation by, *e.g.*, the issue of relief funds to the unemployed from an increase in the fiduciary note issue. The object is not to raise the price level of consumers' goods to some previous level, but simply to ensure that until normal reinvestment and new investment recommence no further fall will take place. Consumers' subsidies at the expense of increased taxation or an increased public debt are likely to defeat their end since the net estimated return on investment will undergo little change. For this reason grandiose schemes of public works are to be avoided ; their continued maintenance may involve, at a later date, a potential increase in taxation. But there seems no reason why the extensive renewal of well established public services, *e.g.*, roads, should not be encouraged.

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[1] The technical difficulties of central bank participation are great. Fixed trust organisations may in the future provide an outlet for extended open market operations.

Consumers' subsidies in the form of relief to the unemployed through the inflationary expansion of the fiduciary issue have one great disadvantage. Under the " automatic " organisation of the credit structure, an increased fiduciary issue would lead to an expanded basis of credit of the joint stock banks, and one which it might be difficult to offset by the central bank's sale of securities when the demand for loans revived. How far this circumstance should inhibit a desire to right a current situation can only be judged in each particular `case by estimating the volume of relief required. Forecasting the turning-point in the trade cycle is not easy, and although some increase in the fiduciary issue will be necessary from time to time, if the joint stock banks adhere rigidly to a definite reserve ratio, there are considerable dangers attached to consumption inflation. If, therefore, relief funds could be supplemented from other sources (without increasing the public debt) fear on this account would diminish. The only other source is unused government assets. These are in fact usually negligible, but by far-sighted policy during the boom they could be built up for the express purpose of maintaining government expenditure during a future depression, without increasing the burden of taxation. The Government of the Federated Malay States has the distinction of being the first government to pursue a policy of building up assets as an insurance against a falling off in revenue in the future. Oddly enough, this policy has been defended as " orthodox."

An Analysis of Unemployment III

By Sir William Beveridge

VII

Sex and Unemployment

The general rates of unemployment of males and females, as recorded by the Ministry of Labour for the years 1927–1936, have been given and discussed in the first section of this article. In the five years 1932–36, during which there have been no major changes of the insurance scheme affecting the relations of male and female unemployment, the general rate for females has averaged 54 per cent. of the rate for males. This perhaps may be taken as the normal relation on the present basis of insurance. That is to say, in proportion to the numbers insured, the recorded unemployment of women and girls is a little more than half that of men and boys.

Apart from this difference in the general rates of unemployment, there are marked differences between men and women in respect of their relation to the insurance and assistance schemes and of their liability to prolonged unemployment.

The following table shows how 100 unemployed persons of each sex stood in relation to the insurance and assistance schemes at 8th June, 1936:—

Table XVI

CLASSIFICATION OF INSURED UNEMPLOYED AT 8th JUNE, 1936.

	Men		Women		Total	
Claimants to Benefit		44.3		62.8		47.4
Applications for Assistance		44.7		14.3		39.6
Claims under Consideration	2.0		3.6		2.3	
Non-Claimants on Register	6.6		8.4		6.9	
Two Months File	2.4		10.9		3.8	
Neither Benefit nor Assistance		11.0		22.9		13.0
		100.0		100.0		100.0

This table (unlike the figures given in Table I) is limited to Great Britain and to men and women of 18 to 64; boys and girls of 16 and 17 are excluded. Of 100 unemployed men, 44 get benefit, 45 get assistance, and 11 get neither. Of 100 unemployed women, 63 get benefit, 14 get assistance, and 23 get neither.

The incidence of prolonged unemployment is much heavier with men than with women. Table XVII classifies the applicants for benefit or assistance at 8th June, 1936, according to the length of their last period of continuous unemployment. The table is limited to Great Britain, to men and women of 18–64 and to persons claiming benefit or assistance; that is to say it excludes boys and girls and persons not in a position to make claims. Broadly, it covers seven out of eight of the men and women recorded as unemployed.[1]

Of every 100 unemployed men claiming benefit or allowance, 26.5 per cent. or more than one in four, and of every 100 women claiming benefit or allowance 9.1 per cent. or less than one in ten, have been out of work continuously for 12 months or more. The last two columns of the table show that, in proportion to the numbers insured, there are, among claimants of benefit or assistance, four times as many men as women, who have been unemployed from nine to twelve months, and more than six times as many men as women, who have been unemployed twelve months or more.

The three distinctions noted above—a lower general rate of unemployment, a larger proportion of this unemployment covered by benefit, and a much smaller liability to prolonged unemployment among women than among men —appear when the insured persons of each sex are taken as a whole, with all the industries lumped together. It is notorious, however, that several industries most affected by the recent depression—coal and iron mining, shipbuilding, marine engineering, pig iron and other heavy metal industries—are almost confined to men. In order to see how far the differences named above are true sex distinctions and how far they arise from industrial differences, it is necessary to compare the unemployment of men and women industry by industry.

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[1] Table XVII covers the same ground as the regular monthly statistics published in the *Labour Gazette* dealing with Periods of Registered Unemployment. (See Section V in ECONOMICA for February, 1937.)

TABLE XVII

MEN AND WOMEN CLAIMING BENEFIT OR ASSISTANCE AT 8TH JUNE, 1936, CLASSIFIED BY DURATION OF REGISTERED UNEMPLOYMENT (G.B.)

Claimants who had been on the Register	Men 18–64		Women 18–64		Women as percentage of men	Rate per cent. of all insured at July, 1935	
	Number	Percentage of all men claiming	Number	Percentage of all women claiming		Men	Women
Less than 3 months	610,027	49.7	144,749	67.6	23.7	6.9	4.7
3 months but less than 6	134,834	11.0	30,200	14.1	22.4	1.5	1.0
6 months but less than 9	90,221	7.4	12,647	5.9	14.2	1.0	.4
9 months but less than 12 ..	66,456	5.4	7,073	3.3	10.6	.7	.2
12 months or more	325,304	26.5	19,474	9.1	6.0	3.7	.6
Total	1,226,842	100.0	214,143	100.0	17.4	13.8	6.9

For this purpose it is desirable to concentrate attention on those industries in which both men and women are employed in substantial numbers. In all the extractive and in some of the manufacturing industries, the proportion of women employed is so small, that fair comparison between the sexes cannot be made. An examination has been made accordingly of those manufacturing industries in which the women and girls were as much as one quarter of the total of insured persons in July, 1935. These industries—44 in number—include nearly 90 per cent. of all the women and girls employed in manufacturing ; the males and females in them are roughly equal. A detailed table has been prepared showing for males and females respectively in each of these industries the numbers insured at July, 1935, and the mean unemployment percentage from June, 1935, to May, 1936, and in June, 1936, and for men and women respectively in June, 1936, the proportion of the unemployed claiming benefit, assistance or neither, the proportions continuously unemployed for twelve months or more, and the proportions continuously unemployed for nine months and less than twelve months.

This table has appeared to be too long for printing here. Its results for the 44 manufacturing industries as a whole are summarised in Table XVIII and compared with corresponding figures for all industries. The examination summarised in Table XVIII is confined to manufacturing industries. The only other industries in the Ministry of Labour classification with females forming as much as one quarter of the insured persons are laundries, job dyeing, dry cleaning, etc., and the five very miscellaneous groups of distributive trades ; commerce and finance ; professional services ; entertainments and sports ; hotel, public house, etc., service. Corresponding figures to those in Table XVIII would show that in each of them the rate of unemployment is greater among men than among women, the proportion of unemployment covered by benefit is greater among women, and the proportion of women getting neither benefit nor assistance is notably high. In those respects the figures for these miscellaneous industries present the same picture as that of the manufacturing industries in Table XVIII and in some other respects a different picture. But the size and character of most of these "industries" makes direct comparison of the position of men and women of little value. In the rest of this paper they are left out of account.[1]

TABLE XVIII

UNEMPLOYMENT OF MEN AND WOMEN

	44 Manufacturing Industries	All Insured Industries
Insured Persons at July, 1935—		
Males	1,703,080	9,531,000
Females	1,826,020	3,527,000
Females as Percentage of Total	51.7	27.0
Unemployed Percentages June, 1935–May, 1936—		
Males	13.9	16.8
Females	11.0	9.4
Unemployed Percentages June, 1936—		
Males	11.5	14.6
Females	10.0	8.1
Proportion of Unemployed at 8th June, 1936:		
Claiming Benefit—		
Men	54.6	44.3
Women	68.2	62.8
Claiming Assistance—		
Men	33.8	44.7
Women	11.5	14.3
Neither Benefit nor Assistance—		
Men	11.4	11.0
Women	20.3	22.9
Proportion of Applicants for Benefit or Assistance at 8th June, 1936:		
Unemployed for 12 months or more—		
Men	19.6	26.5
Women	7.3	9.1
Unemployed for 9 months and less than 12 months—		
Men	4.8	5.4
Women	2.8	3.5

Note: The numbers of insured persons and unemployment percentages relate to persons aged 16 to 64 in Great Britain and Northern Ireland. The proportions in the lower part of the table relate to men and women aged 18 to 64 in Great Britain.

It will be seen that in these 44 manufacturing industries there is much less difference between the rates of unemployment of men and women than there is when all insured industries are lumped together. The weighted means of the unemployed percentages of the 44 industries in June, 1935–May, 1936, are 13.9 for males and 11.0 for females, as compared with 16.8 and 9.4 in all industries: the rate for females is 79 per cent. of that for males in the former and 56 per cent. in the latter. In 12 of the 44 industries taken separately, the females actually have more unemployment than the males; these twelve industries include cotton weaving, jute, the making of electric cables and apparatus, hosiery, carpets, hats and caps, and wall-paper. On the other hand, in cotton-spinning, tailoring, printing, and bookbinding and the making of boots and shoes and other industries the rates for males are substantially above those for females.

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[1] Since this paper was first written, statistics of unemployment have begun to be available for yet another industry—that of agriculture—as the result of the special insurance scheme introduced by the Act of 1936. For the first four months of full operation of the scheme, from November 1936, to February 1937, the mean unemployment percentage in Britain has been 3.7 for males, 19.4 for females, and 4.6 for both together; that is to say, the rate of unemployment among females has been five times the rate among males. This is a remarkable result, on which, however, it is impossible to form a judgment without a full year's experience.

In June, 1936, the sexes are even closer together. The weighted means in the 44 manufacturing industries are 11.5 for males and 10.0 for females, that is to say, the rate for females is 87 per cent. of that for males. Of the individual industries, 25 show more unemployment among males and 18 show more among females, while in one case the rates for the sexes are equal. This closer approximation of the position of males and females in June, 1936, as compared with the preceding twelve months, is probably in part seasonal, and in part due to the continued progress of trade recovery, still further improving the position of men relatively to women. Whatever the cause, in that particular month, in manufacturing industries in which men and women are both employed in substantial proportions,[1] it is almost as common for the women to have a higher rate of unemployment than that of the men as to have a lower rate.

In striking contrast to this varied picture of the relative rates of unemployment among males and females, the columns of the detailed table (unprinted) showing the proportions of this unemployment that rank for benefit give a uniform result. In every industry, the proportion of unemployment covered by insurance is greater among women than among men. On this second point, the figures for separate industries confirm, in place of correcting, the figures for all industries lumped together.

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[1] The detailed table includes three industries—dressmaking and millinery, shirts, collars, etc., and hosiery—in each of which the females form more than 75 per cent. of the insured persons, so that the males form less than 25 per cent. In the first of those the rate of unemployment in June, 1936, is lower among females than among males, and in the second higher, while in hosiery the rates for the two sexes are equal. The omission of these industries—on the ground that in them the proportion of men is not substantial—would not affect the conclusions drawn.

This holds equally where the proportions covered by benefit are relatively low, as in cotton, jute, hemp, rubber or chemicals, and where they are relatively high, as in the other textiles, and most of the clothing industries. It holds, without exception, even in all the 19 industries where the rate of unemployment is higher for women than for men. The weighted means of the 44 percentages giving the proportions of unemployment covered by benefit are 54.6 for men and 68.2 for women.

These proportions in the lower part of Table XVIII do not cover exactly the same ground as the unemployed percentages in the upper part. The former relate to Great Britain and only to men and women of 18 and upwards. The latter include also Northern Ireland (with a total insured population about 2.1 per cent. of that in Great Britain) and boys and girls of 16 or 17 (about 6.4 per cent. of the insured population). There seems to be no reason for supposing that the inclusion of Northern Ireland and of boys and girls would alter appreciably the proportions in the lower part. If these proportions are treated, accordingly, as applying to the unemployed percentages above them, the unemployed percentage on benefit in the 44 manufacturing industries in June, 1936, works out at 6.3 for males and 6.8 for females. If the proportions are applied to the unemployed percentages for June, 1935–May, 1936, in columns 5 and 6, the unemployed percentage on benefit in that period works out for these industries at 7.6 for males and 7.5 for females.

For the reasons just given, these unemployed percentages on benefit cannot be submitted as accurate. But they are not likely to be far from the truth. And they put the relation of men and women respectively to the scheme of unemployment insurance in a new light.

The great difference in the general rates of unemployment, as between males and females, is not infrequently cited as making it unjust to pool the contributions of the two sexes in a single fund; it is suggested that, since the risk of unemployment is so much less for women, they should pay lower contributions in relation to their rates of benefit. This is not the place to discuss the general issue of differentiation of premiums according to risk, in unemployment insurance; strong arguments can be advanced on each side of this issue. In respect of the two sexes, it is sufficient to point out that hasty inferences from their general rates of unemployment have to be corrected by two considerations.

First, the difference of these rates is largely, though not wholly, due to the nature of the industries in which men and women engage. In the manufacturing industries in which men and women are both employed substantially, the women's rate of unemployment is not about 54 per cent. of the men's rate but about 80 per cent. To some extent, the argument for differentiation of premiums by risk must be one for differentiation between industries rather than between sexes.

Second, since unemployment insurance is designed to cover, not all unemployment, but only part of it, the rates of unemployment to be taken account of in comparing the risks are the rates of unemployment on benefit, not the rates of all recorded unemployment. The conditions of the insurance scheme in fact admit to benefit a larger proportion of the recorded unemployment among women than among men. In the six years 1930 to 1935 the mean rate of recorded unemployment in all industries was 21.6 for males and 13.7 for females. But the rates of unemployment on benefit, that is to say the percentages of insured persons able to draw benefit, were much closer together: the rate for males was 8.2 per cent. and that for females 6.9 per cent. In the 44 manufacturing industries specially examined above it seems likely that the rate of unemployment on benefit is actually higher among women than it is among men. In several of these industries taken individually, a rate of recorded unemployment among women equal to or higher than that of men is found in combination with a larger proportion on benefit. Pottery, cotton weaving, hosiery, silk, jute and several other textiles, hats and caps, shirts, collars, etc., cocoa, chocolate, etc., tobacco, explosives, are all in this position in June, 1936.

One explanation—probably the chief explanation—of the larger proportion of women's unemployment that is covered by benefit, is suggested by the last two columns of the table, showing the incidence of long-period unemployment. On this third point, as on the second, the figures for separate industries agree with those for all industries lumped together. In every one of the 44 manufacturing industries examined, the proportion of those unemployed at 8th June, 1936, who had been continuously unemployed for twelve months or more, is higher for men than for women. This holds, without exception, even where the rate of unemployment is less among the men. The difference, moreover, in nearly all cases is substantial. In four industries only, out of the 44, is the proportion of this long-period unemployment less than 10 per cent. among men; in nine cases only is it higher than 10 per cent. among women. The weighted mean of the 44 percentages for men is 19.6, nearly one in five; the corresponding figure for women is 7.3, practically one in fourteen. With only five exceptions out of the 44, the same relation holds for unemployment from nine to twelve months in duration; that is to say the proportion is higher for men. Industry by industry, men are far more subject to prolonged unemployment than women. Unemployment of twelve months or more accounts for most of the difference found above in the unemployment rates of men and women in the 44 industries. If this long-period unemployment is excluded, the general level of unemployment appears to be substantially the same for both sexes in these industries.[1]

Why should men be more subject to long-period unemployment than women? There is probably more than one reason, but one reason at least is obvious. Through retirement on marriage, the age distribution of insured women is different from that of men. Of every 100 insured males more than 25, and of every 100 insured females only eight, are aged 45 and upwards. A much smaller proportion of women in industry than of men in industry are subject to the special difficulties of older people in recovering employment, once they have lost it. The heavy incidence of prolonged unemployment after middle age appears in a new setting, as a difference between men and women.

Apart from marriage, it is possible that women who lose industrial employment and find it difficult to recover are more apt than men to sever their connection with the insurance system. Some may find unpaid domestic occupation—helping with the work of a household where the wife is dead or ailing or occupied. Others take to outdoor domestic service; this is the one large occupation now excluded from insurance and is practically confined to women.

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[1] The returns as to duration of unemployment cover only persons claiming benefit or assistance, i.e. over 90 per cent. of all the men and 80 per cent. of all the women recorded as unemployed. If it could be assumed that those excluded were distributed by duration in the same way as those directly covered by the returns, those unemployed for less than 12 months would be 80.4 per cent. of 11.47 per cent., i.e. 9.2 per cent. of the insured males and 92.7 per cent. of 10.14 per cent., i.e. 9.4 per cent. of the insured females, in the 44 manufacturing industries in June, 1936. This calculation is illustrative rather than accurate, but points to the truth of the statement in the text.

Women are relatively more successful than men in covering their unemployment by insurance benefit under Part I of the Unemployment Act of 1934. They appear relatively much less than men under Part II of the Act, as successful claimants for assistance from the Unemployment Assistance Board. 62.8 per cent. of all unemployed women in June, 1936, had claims admitted to benefit as compared with 44.3 per cent. among men. But only 14.3 per cent. of the unemployed women had claims admitted for assistance, as compared with 44.7 per cent. with such claims admitted among men. This left the proportion of those having neither benefit nor assistance higher among women than among men, 22.9 per cent. of the whole as compared with 11.0 per cent.

The examination whose results are summarised in Table XVIII shows that this holds true of each separate industry as well as of all industries lumped together. In every one of the 44 manufacturing industries, there are proportionately fewer women on assistance and there are proportionately more women than men who, though unemployed, obtain neither benefit nor assistance. For men, the mean of the 44 percentages of those in this position (i.e. without benefit or assistance) is 11.4; for women it is 20.3. These figures are not very different from the corresponding proportions for all insured men and all insured women taken as a whole, namely 11.0 and 22.9.

As appears from Table XVI above, there is a marked difference in the proportions of men and women respectively in the "two months file", i.e. among persons who after lodging their books cease to register and are not known to be dead, in employment or sick. This file includes a substantial proportion of cases in which benefit has been disallowed. It is a reasonable hypothesis, pending further enquiry, that the relatively greater number of women here represents largely the operation of the Anomalies Order in respect of married women. These women are disallowed benefit on the ground that they are not really in the industrial field, but for two months after disallowance nearly all of them are included in the statistics of unemployment.

It is possible that another factor contributing to the relatively small proportion of women seeking assistance and the resulting large proportion of non-claimants is that, on the whole, the earnings of a woman are more likely to be subsidiary to the main income of a household than the earnings of a man. The Means Test may operate accordingly to exclude a larger proportion of women than of men, and women, knowing this, may be less likely to make claims for assistance.

In raising such issues, I am reaching the limits where purely statistical enquiry ceases to be fruitful. It must be supplemented, here as in the social sciences generally, by field work.

It is possible, for instance, that the lesser incidence of long-term unemployment among women than among men is due not simply or mainly to the cause suggested above —to the statistical fact of different age distributions among insured men and women respectively. There may be another explanation: that women are more resistant to the demoralising influence of prolonged unemployment than men. If they lose their industrial jobs, they can more readily find occupation, if not income, than men, because they can always find something to do at home. They are less likely to be wholly idle and as a consequence may become unemployable more slowly: they may remain better fitted than men to return to their industrial jobs whenever a chance offers. This suggested explanation is a hypothesis only, but is one well worth testing. Its testing requires not statistical analysis but personal investigation in the field.

It is possible, again, that the marked differences between men and women in type of recorded unemployment and in relation to insurance and assistance, reflect, in the last resort, differences of attitude towards employment for wages. For several reasons, it may be suggested, employment for wages is more important to men than it is to large numbers of women. This makes it more possible for women than for men to adjust their lives to the conditions of the insurance and assistance schemes: to alternate between insured employment and domestic occupation in such a way as to benefit as fully as possible by insurance; to cease to register as unemployed when neither benefit nor assistance can be got by it. This again is a suggestion for exploration by methods other than statistical. This exploration I must leave to others.

I conclude by summarising the tentative conclusions that appear to emerge from the facts marshalled above, as to the bearing of sex on unemployment as defined and recorded in the statistics of the Ministry of Labour.

1. The higher general level of unemployment among insured males as a whole than among insured females is a distinction less of sex than of industry. The industries of heavy unemployment to-day are, in the main, industries in which women are little occupied. In those manufacturing industries in which men and women are both employed in substantial proportions, men are sometimes more liable to unemployment than women and sometimes less.

2. There remains, even in these manufacturing industries, on balance, a higher rate of unemployment among men. But this appears to be little more than can be accounted for by the heavier incidence on men of long-period unemployment, which in turn can be explained, in part at least, by the smaller proportion of women than of men who remain in industry after middle age. Whether it can be wholly explained in this way is not yet certain. It is clear, at any rate, that, apart from industry and age, there is no great difference between the unemployment rates of the two sexes; there is nothing to suggest, for instance, that women, as such, are less liable to unemployment than men, because they are less strongly organised in trade unions.

3. The lighter incidence of long unemployment upon women, the larger proportion of their unemployment that is covered by benefit, and the larger proportion for which they obtain neither benefit nor assistance, appear to be true sex distinctions, being found in each industry taken separately as well as when all industries are lumped together.

4. The explanation of these differences calls for further exploration by direct enquiry. But it seems likely that in part they reflect, not economic conditions as that term is commonly understood, but differences in the attitude of men and women towards employment for wages.

VIII

Conclusion

In presenting to the British Association in September, 1936, the paper which forms the basis of these articles, I summarised my outlook for unemployment in the following terms:

"For different purposes unemployment can be classified in different ways. If our purpose is to get a picture of the unemployment for which provision should be made in future, classification under three main heads is convenient.

First, there is short-period unemployment due to friction in the labour market and to seasonal fluctuations. This cannot safely be put at less than 6 per cent. and may be as high as 8 per cent.—say between 800,000 and 1,000,000 unemployed.

Second, there is long-period unemployment due either to large and rapid structural changes in industry (the 'industrial hard core' of unemployment in Northern Britain and Wales) or to personal infirmities—of age or physical capacity or character (producing a 'personal hard core' of unemployment both in prosperous and unprosperous regions). The bulk of the 400,000 claimants to benefit or assistance who even in the prosperity of July, 1936, had been unemployed continuously for nine months or more, and of the 200,000 persons with unemployment books who were then unable to claim either, fall in one of these categories—say 500,000 altogether or 4 per cent. of the insured population outside agriculture.

Third, there is cyclical unemployment in trade depressions. More accurately, there has been cyclical unemployment (from 1931 to 1933) and in all probability will be again; at this moment it is practically non-existent. No one can do more than guess at the time when depression is likely to return or how severe it will be. But it would not be prudent to allow for an increase in unemployment of less than 6 per cent.—say 800,000—or to assume that it can be many years distant.

Frictional, seasonal and cyclical unemployment form the natural field for unemployment insurance—a spreading of wages over good times and bad. The Unemployment Fund even to-day is paying out between £600,000 and £700,000 a week for current unemployment and is accumulating a reserve—now over £30,000,000—against depression. For long-period unemployment, whether due mainly to structural or mainly to personal causes, something other and more than money payments in idleness is required."

This statement was translated and widely reported on both sides of the Atlantic as a prophecy that the number of the unemployed in Britain would rise almost immediately to 3,000,000. It is now placed on record as a belated correction. It is of interest to consider how far, if at all, the outlook has been modified by later developments.

Since the statement was made unemployment, having fallen in August to 11.9 per cent. in Britain, remained at that point till the end of 1936 and, in the opening months of 1937, has shown signs of a further fall. The Unemployment Fund reserve has passed £40,000,000 and the Unemployment Insurance Statutory Committee has for the third time declared a continuing surplus, making with the two previous surpluses a total improvement of £10,000,000 a year, as compared with the expectations on which unemployment insurance was based in the Act of 1934. The rearmament programme has come to crown and carry on the general recovery of trade.

In one respect—that of time—the rearmament programme modifies earlier expectations, though not the actual wording of the statement quoted above. It is bound to prolong recovery and to postpone, though not indefinitely, any downward turn of employment. In other respects, the picture presented last September appears still the most probable view of unemployment. Rearmament expenditure, in so far as it involves a demand for labour specialised on industries such as shipbuilding which had been structurally depressed or is directed deliberately to the special areas, may eat into the 4 per cent. of long-period unemployment. It may bring frictional and seasonal unemployment to the lower, rather than to the higher, of the figures named, to 6 per cent. rather than to 8 per cent. But I should be surprised if it did much more than that. The absorption into regular industry of the chronic unemployed of the depressed areas will encounter many personal difficulties, even if there is an unsatisfied demand for labour in those areas. And already, at 6 per cent. or more of unemployment, in many regions of Britain, there are widespread complaints of shortage of labour and delays in production.

Whatever the figure to which unemployment is destined to fall in the near future—whether or not it be below 10 per cent.—there can be no expectation of reaching such percentages as were recorded by trade unions before the war for their own members; these percentages fluctuated between a minimum of 2 per cent. and a maximum of 8 per cent to 10 per cent. with an average, over fifty years, of 4½ per cent. The general level of unemployment, as recorded since the war for the 13,000,000 insured persons, is clearly destined to remain much higher than the level of unemployment recorded before the war among the 1,000,000 or so persons covered by trade union returns. Is the difference one of facts or of record? In proportion to the numbers available for employment is there, since the war, more unemployment than before the war, or is it merely that the record is more complete, that men are counted now as unemployed who, though standing idle, would not have been counted before? The answer, as might be expected, is that there is some difference both in the facts and in the recording of them, though an answer covering all the ground cannot yet be given.

Some of the long-period unemployment—that arising through structural change in the depressed industries and special areas—represents beyond question a difference of fact: more unemployment and not merely a fuller record. The structural changes induced by the war have been of exceptional violence.

Some, both of the long-period unemployment—arising through personal disabilities—and of the short-period unemployment—in intervals between jobs—is as certainly a difference of record rather than of facts.

The unemployment which has been analysed in these articles is unemployment according to a particular definition; the definition varies from time to time as the administration of unemployment insurance varies. Occasionally, as under the Anomalies Acts, a change of administration has tended to strike out of the record of unemployment people who formerly would have appeared there. On the whole, the tendency has been in favour, not of exclusion, but of additions to the record. Many are counted among the unemployed now who would not have appeared, even if they were trade unionists, in the trade union returns of unemployment before the war; many are included now, who would not have been recorded as unemployed even ten years ago. The comparison in the first of these articles, of unemployment among men and women respectively in one particular industry can be paralleled in other industries; the abolition in 1930 of the "genuinely seeking work" condition, uncorrected in the case of men by the Anomalies Act, has added to the record of unemployment among men. Again, the provision introduced in July, 1928, for franking the health insurance cards of persons proving unemployment has given a new motive to registration as unemployed. Under precisely the same conditions of unemployment, the recorded percentage of unemployment would be higher to-day than in 1927. Conversely, the same recorded percentages in 1927 and 1937 would mean that the actual unemployment in 1937 was less. Even between the time when this is being written, and the time when it will be published, it is likely that other administrative changes, namely, the coming of the second appointed day extending assistance to cover new classes of persons, and the shortening of the waiting period for benefit will swell still further the record of unemployment.

No numerical estimate is attempted here of how much of the apparent increase of unemployment since the war is due simply to better recording. Such an estimate should not be impossible, but must be left for later study. With it must be left two questions of great importance.

First, is there any reason for supposing that, apart from the structural changes which have depressed particular industries, there is likely now, even at the top of the trade cycle, to be more unemployment than before the war? In other words, is there more friction in the labour market, rather than less?

Second, is there any reason for supposing that cyclical depressions in future will differ from those before the war, either in the direction of greater severity or of less severity?

As a matter of speculation, rather than of assurance, it is reasonable to answer the first question in the affirmative. It is tempting and probably safe, as a confession of ignorance, to answer the second question in the negative and to await the event.

The New Conditions of Entry Into the Road Haulage Business

By G. J. Ponsonby

UNDER the provisions of the Road and Rail Traffic Act, 1933, there was instituted a system under which " no person shall use a goods vehicle on a road for the carriage of goods . . . except under a licence."[1] The Act conferred wide powers of discretion upon the licensing authorities concerned, both as regards the granting of licences to those already established in the haulage industry and to those " newcomers " desirous of entering the industry for the first time. It is proposed here, first, to record summarily those provisions of the Act which refer to the system of licensing. There will then follow an account of the manner in which the licensing authorities' discretion has been exercised in so far as it affects the conditions of entry for " newcomers " to the industry, *i.e.*, those who were not actually carrying goods for hire or reward during the year ended March 31st, 1933.

The persons in whom is vested the power of granting, refusing, or attaching conditions to licences under the Act are the chairmen[2] of the Traffic Commissioners, who, under the Road Traffic Act of 1930 are responsible for the issue of public passenger service vehicle licences. There are at present twelve of these Licensing Authorities, having jurisdiction respectively over that number of Traffic Areas.[3] If any person is aggrieved by a decision of a Licensing Authority, there is a right of appeal to an Appeal Tribunal which consists of a chairman and two other members, all of whom are appointed by the Minister of Transport.[1]

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[1] 23 & 24 Geo. 5, Ch. 53, Section 1 (i).

[2] In the case of the Metropolitan Traffic Area there is only one Traffic Commissioner. Hence, it is not the Chairman of the Traffic Commissioners, but simply the Traffic Commissioner who is the Licensing Authority in question.

[3] Under the Road Traffic Act, 1930, England, Scotland and Wales are divided into twelve Traffic Areas, over each of which (with the exception of the Metropolitan Area) three Traffic Commissioners have powers to grant or refuse public passenger vehicle licences. One of these three Commissioners is appointed as Chairman, and it is he in whom the additional power of granting licences for goods vehicles is vested. They owe their appointment to the Minister of Transport.

Three classes of licences are issued under the Act as follows :—

1. Public Carriers' Licences ('A' licences).
2. Limited Carriers' Licences ('B' licences).
3. Private Carriers' Licences ('C' licences).

An 'A' licence holder is entitled to use his authorised vehicles for the carriage of goods for hire or reward only, unless the goods are for, or in connection with, his business as a carrier. 'C' licences, on the other hand, entitle holders to use the authorised vehicles only "for the carriage of goods for, or in connection with any trade or business carried on by him," but do not permit the vehicles to be used for hire or reward. The holder of a 'B' licence, however, is entitled to use his authorised vehicles both for the carriage of goods in connection with his own business and also for hire and reward.

With a view to overcoming such ambiguities as would otherwise have arisen as to what precisely constitutes the carriage of goods for reward as distinct from their carriage in connection with any trade or business carried on by the holder, certain definitive clauses were inserted. The latter give a somewhat liberal interpretation as to what is legally regarded as carrying goods in connection with the holder's trade or business ; so that even in cases in which an extra and specific charge is made "the delivery or collection by a person of goods sold . . . in the course of a trade or business carried on by him . . . shall not be deemed to constitute a carrying of the goods for hire or reward." Thereby, it is not necessary to obtain an 'A' licence either for the operation by a tradesman of his delivery vans, or for vehicles used for the delivery (say) of furniture by manufacturer or vendor to householders from factory or shop : a 'C' licence suffices.

To agriculture is granted what amounts to a definite exemption from the general provisions of the Act, for in such cases as when a 'C' licence is required, the farmer is permitted to carry goods for reward though only so long as those goods are for, or in connection with, the business of agriculture and are carried "in the same locality."[1] Under a decision of the High Court the business of agriculture does not include the carriage of the produce of a farm for retail distribution. It may be added that the performance by a local or public authority of its legal functions is deemed to be the carrying on of a business.

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[1] It is laid down in the Act that the Minister shall only appoint the Chairman after consultation with the Lord Chancellor, and shall only appoint the other two members after consultation with the President of the Board of Trade and with the Secretary of State for Scotland.

According to the First Annual Reports of the Licensing Authorities, which covered the period 1st January, 1934, to 30th September, 1935, but were not published until March, 1936, the number of vehicles authorised under the three classes of licences issued was at 30th September, 1935, as follows:—

Vehicles authorised at 30th September, 1935.

'A' licences..	100,182
'B' licences..	55,558
'C' licences..	303,886

II

The extent to which the Licensing Authority has discretion to grant or refuse an application for a licence or to attach conditions thereto, varies considerably as between these three classes of licence in question.

Taking first the case of the ancillary users ('C' licences), "unless the applicant is the holder of a licence which is suspended, or unless a licence previously held by him has been revoked," the application shall be granted whether the firm in question is already operating a fleet or desires to do so for the first time. This means that so long as a would-be ancillary user maintains his vehicle in a state of fitness, and complies with regulations appertaining principally to safety, such as limits of speed, weight, and the loading of vehicles, and so on—conditions which are applicable to all licence-holders—he is free both to start operating and to increase his fleet without let or hindrance. This freedom has been regarded as a safeguard for industry against any monopolistic tendencies that may arise as the result of other parts of the Act.

With 'A' licences, also, there are circumstances in which the Authority has no power to withhold a licence. A licence must be granted in such cases where the authorised vehicles are to be used exclusively on a contract made between a haulier and *one other* person for the carriage of the latter's goods, and where the contract is to carry for not less than one year. Prior to the passing of the Act such cases would have existed where a firm had desired transport of such a character that one or more suitable vehicles were fairly intensively employed throughout the year, but where for some reason or another the firm in question preferred to hand over the work to those who specialised in transport, rather than make transport a part of their business themselves.

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[1] This special exemption, coupled with the absence of any precise definition as to what constitutes "in the same locality" has, according to one Traffic Commissioner, ". . . caused considerable dissatisfaction to 'A' and 'B' licence-holders, who allege that their legitimate business is being filched by farmers who have Biblical rather than legal notions of what constitutes a neighbour" (p. 57. *First Annual Report of the Licensing Authorities*).

Secondly, also, in the case of 'A' licences, an application had to be granted *for the first currency period*[1]: that is, the first period of two years for which a claim for a licence was made, so long as the aggregate unladen weight of the vehicles concerned did not exceed that weight owned and used mainly for hire or reward, by the applicant during the 'basic' year, *i.e.*, the year ended 31st March, 1933. Such unladen weight or tonnage, to which carriers had an absolute right under the Act, became known as "claimed tonnage." If, however, a carrier has desired to expand his business (in terms of unladen weight of vehicles owned) over and above that which he had owned in the basic year, the Licensing Authority has had power to prevent such developments. This additional tonnage is known as "discretionary tonnage." At the expiration of the first currency period all 'A' and 'B' licence tonnage became "discretionary" in the sense that all licences have been granted at the discretion of the Authority, and none "as of right" under the Act. As regards the granting of 'A' licences to those who were not in the business at all during the basic year, subject to the "contract" licence provisions, the Licensing Authorities have possessed absolute powers to grant or refuse from the start.

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[1] The licensing provisions of the Act for 'A' and 'B' licences became operative on October 1st, 1934. For reasons connected with the administration of the Act, all 'A' licences were not actually granted as from that initial date. Short-term licences, issued for periods not exceeding one year, were first granted, the issue of full-period (two-year) licences being spread over the year ended 30th September, 1935. Thus, the first currency period for 'A' licence vehicles did not expire for all vehicles on October 1st, 1936, but are becoming due for reconsideration during the (present) year following that date. Similar circumstances have arisen in the case of 'B' licences, although in this case short-term licences were not permitted to exceed six months, the currency period being one instead of two years.

Somewhat similar provisions applied to the grant of 'B' licences during the first currency period. In this case, also, so long as it was proved that the tonnage for which a licence was being claimed was owned in the basic year, and "partly used for the purpose of the carriage of goods for hire or reward and partly for other purposes," the licence had to be granted. But it was added in this case *à propos* of the discretion possessed by the Licensing Authorities in these cases to attach conditions thereto, that they shall not attach any conditions "which would constitute a substantial interference with the carrying on of any trade or business for the purposes of which the vehicles were used. . ." Again, the Licensing Authorities have had absolute authority from the start to grant or refuse licences to those who, desiring to hold a 'B' licence, were not actually engaged in carrying both for their own business and also for hire and reward during the basic year.

Broadly, we may say that the discretion or veto of the Licensing Authority during the first currency periods extended in the case of 'A' and 'B' licences to any expansion of tonnage over and above that which was possessed in the basic year, whether desired by newcomers to the industry or by those already established, and in the case of 'B' licences not only to the expansion of tonnage, but, in addition, to attaching conditions delimiting their freedom to ply for hire both as regards the places served, the goods carried, and the business houses catered for; but that since the expiration of the first currency periods, both for 'A' and 'B' licences, there has been an absolute discretion over all 'A' and 'B' licence tonnage. It is, perhaps, worth noting at this stage that in no case can the Licensing Authority attach conditions as regards rates charged.

The subject matter of this article is the terms on which licences have been granted to those who, desiring 'A' licences, were not actually operating during the basic year, *i.e.*, the year ended 31st March, 1933, and who came to be known as 'newcomers' to the industry.[1]

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[1] It is important to notice that the term 'newcomer' refers to anyone who, desirous of holding an 'A' or 'B' licence, was not actually operating a vehicle during the basic year. All those, therefore, who happen to have come into the business between March 31st, 1933 (the end of the basic year) and October 1st, 1934, when the licensing provisions of the Act came into operation, have been regarded as 'newcomers' *à propos* of obtaining a licence.

III

Before giving an account of these terms of entry, a brief examination of the clauses in the Act which instruct the Licensing Authorities as to the manner in which they should exercise their discretionary powers will be made. At least two sections in the Act may be construed as bearing on the subject. First of all, Section 11 (2), which applies to applications both for 'A' and 'B' licences, runs as follows :—

> " It shall be the duty of the licensing authority, on an application to which this section applies, to take into consideration any objections to the application which may be made by persons who are already providing facilities, whether by means of road transport or any other kind of transport, for the carriage of goods for hire or reward in the district, or between the places, which the applicant intends to serve, on the ground that suitable transport facilities in that district, or between those places, are, or, if the application were granted, would be, either generally or in respect of any particular type of vehicles, *in excess of requirements*,[1] or on the ground that any of the conditions of a licence held by the applicant has not been complied with :
>
> " Provided that on an application for the grant or variation of an 'A' licence, the licensing authority shall not be bound to take into consideration objections made by a person who holds a 'B' licence and does not also hold an 'A' licence."

Some idea of the importance of this clause may be gathered from the words of the Minister in charge of the Bill in Committee, when he successfully resisted an amendment to eliminate the words " or any other kind of transport," which would have had the effect of preventing railway companies from inclusion among the objectors to whose representations the Licensing Authority is bound to have regard. He said : " I think the hon. Member can hardly have expected me to accept this amendment, which goes to the root of the Bill. The effect of the present amendment would . . . cut out such people as the railway companies, canal companies, or coastwise shipping companies from any *locus* or objection at all, and would, therefore, defeat even the modest beginning which I hope this Bill will make towards some form of a division of function between the various forms of transport."[1]

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[1] Italics mine.

Another section bearing directly upon the subject, and which applies to applications both for 'A' and 'B' licences, is Section 6 (2) of the Act, which reads as follows:—

> "The licensing authority in exercising his discretion shall have regard primarily to the interests of the public generally, including those of persons requiring, as well as those of persons providing, facilities for transport, and, in particular, shall have regard in the case of an application for an 'A' licence or for a 'B' licence—
>
> (*a*) where the applicant is the holder of an existing licence of the same class, to the extent to which he is authorised to use goods vehicles thereunder for the carriage of goods for hire or reward;
>
> (*b*) to the previous conduct of the applicant in the capacity of a carrier of goods;

There follow clauses referring to the need for providing for occasions when vehicles are withdrawn from service for overhaul, etc.

It is the fact that these sections cannot be said to specify any very definite policy to be adopted by the Licensing Authorities, that makes an examination of the manner in which they have been interpreted, a matter of such importance.

IV

It must not be taken that the principles or rulings laid down by the Appeal Tribunal on which the following study is based have, in fact, been applied in all cases ever since the Act has been in operation. This has obviously not been the case, some of the principles only having been announced in 1936. The principles have gradually emerged from the hearing of scores of cases heard since the Tribunal started its work in September, 1934. Meanwhile, the Traffic Commissioners in each area have acted independently, interpreting the Act as they have thought fit.

Moreover, as will be seen later, since the facts, the establishment of which may be required for proving a case, are frequently matters of degree, many decisions inevitably become matters of opinion rather than principle. Two conflicting principles are sometimes involved, the ultimate decision to grant or not to grant depending upon the relative weight given to conflicting considerations. No one established principle should therefore be taken as ultimately binding apart from the existence of other principles. Nevertheless, once certain principles have been laid down (and some of the most vital principles were, in fact, laid down at the outset, notably those embodied in Enston's and Ridgewell's cases), the Traffic Commissioners have been bound to respect them in the following sense: that in the event of their being manifestly too liberal in their grant (in relation to the principles), those parties whose interests are deemed to be adversely affected can appeal and get the decision reversed; whereas if a Commissioner refuses to grant a licence when, in fact, by the principles laid down by the Tribunal it should clearly have been granted, then again the aggrieved party can get the decision reversed in his favour. Once, therefore, these principles were laid down by the Tribunal, they have in the above sense become binding upon the Traffic Commissioners as a whole.

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[1] *Parliamentary Debate, Commons, Standing Committee A.* 22nd June, 1933, pp. 436–7.

The first and most important condition that must be satisfied in the case of those requiring to enter the haulage industry as a newcomer is as follows:—

> *There must be proof that the haulage work proposed to be done is both required by the public and also could not, for some reason, be done by already existing carriers.*
>
> This principle was laid down in Enston's case as follows:—
>
> ". . . He [the newcomer] must discharge the onus of proving that there are persons ready and willing to employ him, and in addition must lead evidence sufficient to make out to the satisfaction of the Licensing Authority a *prima facie* case that the haulage work which he proposes to carry on, or embark upon, cannot for some reason be done by other operators already engaged in carrying, whether by road or by rail. If he does this, then it is for the objectors, if they can, to rebut the *prima facie* case which the applicant has made."[1]

Many cases in which the applicants have proved that there are persons ready and willing to employ them but have failed to satisfy the Licensing Authorities that the work proposed to be done could not be done by others might be quoted.[1] But the full significance of the principle laid down in Enston's case can only be realised by means of a detailed examination of the manner in which the principle has been applied, and the precise meaning that has been attached to the words "cannot for some reason be done by operators already engaged in carrying."

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol. 22, p. 5.

It must be admitted that the Tribunal has considered it necessary to reserve a considerable degree of discretion to meet particular cases. As the Chairman has said: "In considering whether an applicant has made out a *prima facie* case that the haulage work which he proposes to carry on cannot for some reason be done by other operators, many points have to be considered . . . namely adequacy in number of vehicles, suitability of vehicles and efficiency of operation."[2] Nevertheless, in the course of the numerous cases in which newcomers have been granted or refused entry to the industry by the Appeal Tribunal, certain fairly well-defined conditions have been ruled as constituting the circumstance in which the work to be done "cannot for some reason be done by others"; and an account of those conditions will now be given.[3]

A principal circumstance that must be satisfied before the grant of a licence is made is as follows:—

There must be evidence that persons requiring transport had had, or in the absence of the grant of the licence would have had, difficulty in obtaining the transport facilities they required.

In a case in which the London, Midland and Scottish Railway successfully appealed against a Traffic Commissioner's decision to grant a licence, the Chairman of the Tribunal said: "There was, however, no evidence that any persons requiring transport during the two and a half years the respondent was off the road had had any difficulty in obtaining the facilities they required."[4]

In another case in which Enston's case applied and in which the Tribunal reversed the decision of a Traffic Commissioner to grant an 'A' licence to a newcomer, he said: "In this case there was not even a letter from the colliery company or from any other person saying that they had, or would have had, any difficulty in obtaining established hauliers to do their work."[1]

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol 22, p. 6 and p. 149. *Ibid.*, Vol. 23, p. 180, etc.

[2] *Ibid.*, Vol. 22, p. 182.

[3] The cases that have been examined are all those that came before the Tribunal up to the end of July, 1936.

[4] *Ibid*, Vol. 22, p. 187.

Similar considerations obviously carried weight in a case in which the L.N.E.R. Co. successfully appealed against the grant of an 'A' licence and in which the Chairman said, referring to a period of time prior to the application: ". . . there was no evidence that during that period they [the persons for whom the respondent proposed to carry] had any difficulty in obtaining the transport they required."[2]

A second circumstance closely allied to the first, in which a newcomer may not enter the industry, is when it is held by the Licensing Authority that the facilities already being provided are "at least as good" as those proposed to be given. Thus, in a case in which two of the main line railway companies appealed against the grant of an 'A' licence to one who had provided facilities for long-distance furniture removals from Liverpool, the Chairman of the Tribunal said (*inter alia*): "We are . . . of the opinion that the evidence called for the appellants [*i.e.*, the railways] proved that the facilities provided by them for long-distance furniture removals were at least equal to the facilities offered and proposed to be offered . . ."[3] The appeal was allowed in spite of a statement by one of the respondent's customers to the effect that "the facilities offered by the railway companies for these long-distance furniture removals were not equal to those offered by Mr. Williams [the respondent] and his lorry."[4]

Yet another variation of the same underlying principle was laid down in Petrie's case,[5] namely that: "The proved fact that certain firms have made use of the appellant's vehicles is not evidence that there are not available for [their] use vehicles belonging to existing operators *adequate in quantity and suitably and efficiently operated*."[6] Some idea of the meaning that has been attached to these last words may be gathered from the following cases:—

In the *L.M.S. Railway Company and Tait and MacConn* case the former Company unsuccessfully appealed against a decision of a Licensing Authority to grant an 'A' licence in respect of a covered van . . . specially suitable for carrying furniture and other household effects."[1] In the course of giving his decision the Chairman said: "The point on this issue is whether there is available in. Greenock an adequate number of vehicles suitable for the particular work, namely furniture removing, which the respondents propose to embark upon. The evidence is that there are two covered vans available for that class of work. Mr. Wilson [for the appellants] has pressed upon us that during the eighteen months before the respondents made their application, goods belonging to the traders who were ready and willing to employ the respondents in the future must have been carried by somebody. We have no doubt this is correct, but the vehicles (with the exception of the two mentioned) in which they were carried were not suitable for the carrying of the class of goods which those traders wanted to be carried."[2] Thus, in spite of the admission that the traffic proposed to be carried had previously been carried by one class of road vehicle, the latter were deemed as not being suitable for the classes of traffic under consideration. In view of the general attitude adopted by the Tribunal towards "abstracted" traffic,[3] (*i.e.*, traffic that has been carried by already existing carriers), this decision is not unimportant, for it illustrates the point that the principle (elsewhere laid down) that new traffic must not be "abstracted" traffic is not rigidly applicable in all cases.

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol 23, p. 81.

[2] *Ibid.* Vol. 22, p. 148.

[3] *Ibid.* Vol. 23, p. 163.

[4] *Ibid.* Vol. 23, p. 162.

[5] In this case an appeal against a refusal to grant an 'A' licence in the Bristol area was dismissed.

[6] Italics mine.

As regards the word "efficiently," it was laid down in *John Edwards and L.M.S. Railway Company* as follows:—

> "An applicant for an 'A' licence who proves that . . . the service which he offers can be more efficiently operated by him than by others should be granted a licence."[4]

In this case—"The classes of goods mostly carried were plaster mouldings and such like. The plaster mouldings had to be carried to their destination in the mould and removed therefrom on arrival at the site. This traffic was of a special character. The work of removing the mouldings from the moulds was such as required the services of a person experienced in that work. During the time the appellant was in the employ of Adams Brothers he acquired the experience necessary for carrying and dealing with their particular class of traffic."[1] Then, in allowing the appeal :—

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol. 22, p. 201.

[2] *Ibid.*, Vol. 22, p. 203.

[3] See below.

[4] *Ibid.*, Vol. 23, p. 67.

"The evidence of the appellant and the letter from Wolfe and Rowlands in our opinion established a *prima facie* case that the service offered by the appellant could, owing to his previous experience, be more efficiently operated by him than by others, because of the special character of the traffic to be carried by the appellant."[2] It appears from this decision that if a newcomer to the industry can satisfy the Authority both that the traffic to be handled is of a special character, in the sense that it requires a special experience or training to handle it, and that the appellant has had that special experience or acquired that special training, then the service to be rendered will be regarded as being "more efficiently operated . . . than by others."

Further light was thrown upon the significance attached to the word 'efficient' in the case of *Stephenson and Sagar & Son*, in which "the Appeal Tribunal were satisfied that the traffic of the objectors had been captured by the applicant because of the inefficiency of the former."[3] The Tribunal were satisfied that "the reason why his company had taken their work from the respondents was that it had been neglected by them. On many occasions, sometimes three or four times a day, when the respondent's lorry was required to carry bricks from the brickyards to site, Mr. Whittaker had gone to the respondent's premises for the purpose of giving an order and found that the lorry was out delivering coal on its round, and had had great difficulty in obtaining another lorry to do the work."[4] In this case, in spite of the traffic proposed to be carried being "abstracted traffic," the licence was granted. "Inefficiency" had been proved, and "great difficulty" had been experienced in obtaining another lorry to do the work.

Yet another variation of Enston's case demands that there must be "evidence of need or requirement given in support of the application." In a case in which the Appeal Tribunal reversed the decision of a Traffic Commissioner to grant an 'A' licence granted on the ground that it was

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol. 23, p. 68.

[2] *Ibid.*, Vol. 23, pp. 70–1.

[3] *Ibid.*, Vol. 23, p. 183.

[4] *Ibid.*, Vol. 23, pp. 186–7.

"in the interests of the public generally to grant the application."[1] The Chairman of the Tribunal referring to the party who desired the licence, said: "The respondent is a farmer. He stated in his evidence that his proposed venture was to find employment for his son. No evidence was given, nor even a suggestion made that the vehicle proposed to be acquired was required to meet the needs of the traders in Scarborough or elsewhere."[2] Or again: ". . . there was no evidence of need or requirement given in support of the application."[3] Thus, even if there is both a desire to find employment in the industry, and a party "ready and willing to employ," as there was in this case,[4] that does not necessarily prove a need, for "There was . . . no evidence that the persons for whom the respondents proposed to carry could not without this vehicle obtain the transport they required."[5]

The next main principle to be considered is that which lays down that the traffic proposed to be carried must not be "abstracted" traffic; or to put it another way round, the traffic must be new traffic. A case in which this principle was forcibly brought out is *John Rhodes and L.M.S. Railway Company*, in which the appellant, previously an employee in a firm of road hauliers, unsuccessfully appealed against a refusal by a Licensing Authority to grant him a licence. In this case, it was clearly the view of the Tribunal that because the traffic he was carrying was "not new traffic" and had been "previously carried by others," the Licensing Authority had been right in refusing the grant. To quote the Chairman of the Tribunal: "The appellant admitted in evidence before Mr. Chamberlain [the Licensing Authority] that the carrying which he was doing was not new traffic . . . Further, he stated that the traffic he was now carrying had been previously carried by other persons. Mr. Chamberlain in his statement to us says that he came to the conclusion that on such evidence he would not be justified in exercising his discretion in favour of the appellant. We agree."[6]

Another case was that in which an unsuccessful appeal was made against a refusal to grant a desired 'A' licence.

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol. 22, p. 148.

[2] *Ibid.*, Vol. 22, p. 148.

[3] *Ibid.*, Vol. 22, p. 149.

[4] *Ibid.*, Vol. 22, p. 148.

[5] *Ibid.*, Vol. 22, p. 148.

[6] *Ibid.*, Vol. 22, p. 268.

It is clear from this case that the appeal was dismissed because the appellant was attempting to find employment (as opposed to meeting a new need), and that the traffic carried had previously been carried by other transport facilities. In the words of the Chairman: "After losing his work he went round to various firms which were known to him. This could not give him regular work, but he said: 'They told me if I could get a lorry they would give me work, and I did so.' He also said in answer to a question whether it was safe to assume that somebody else was doing the work: 'Somebody else had it.' When we consider these two statements in conjunction we are forced to the conclusion that the appellant was carrying traffic which had previously been carried by the railway companies or by other existing road hauliers. The conclusion we have come to after our own review of the facts is that the appellant did not make out a *prima facie* case that the haulage work which he proposed to carry on could not have been done by other existing operators, and that being our conclusion it follows that the appeal must be dismissed."[1] Clearly, the facts that the traffic carried was "not new traffic"[2] and that it was traffic that "had been previously carried by other persons" helped to decide the case.

A further general condition that must be satisfied by a newcomer, is that:—

> *there must be proof that in the event of the licence being granted, facilities would not be in excess of requirements.*

In the words of the Chairman of the Appeal Tribunal:—

> "On an application for an 'A' licence by a newcomer to the carriers' business, the applicant must not only prove that there are persons ready and willing to employ him, but must also make out a *prima facie* case that if the application is granted traffic facilities in the district which he proposes to serve will not be in excess of requirements."[3]

Upon examination, this second principle does not materially add much to the first principle laid down under Enston's case. Much turns on the definition as to what set of circumstances may be said to constitute "excess" facilities. Two main conditions have so far been deemed to constitute

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol. 22, p. 25.

[2] *Ibid.*, Vol. 22, p. 268.

[3] *Ibid.*, Vol. 23, p. 76.

" excess " facilities. The first of these refers to " abstracted " traffic ; the second, where there is general evidence submitted proving the existence of idle tonnage. Thus, if the traffic carried,[1] or to be carried, is " abstracted " traffic (that is, previously carried by other transport undertakings), then it can be inferred that facilities, in the event of a grant, are, or would be, in excess of requirements. It follows that if an appellant has already been employed by the public, no case has necessarily been made out for the grant of a licence. For example, whereas it was held by Sir William Hart " that where it appeared that a number of years ago a railway company had carried wet hides for a trader who had then transferred his traffic to a road carrier, the inference was that the transport facilities of that [railway] company were not suitable to the requirements of that trader,"[2] on appeal, the Appeal Tribunal held, overruling Sir William Hart, " that the fact that a railway company offered facilities for the transport of wet hides and had a number of years ago carried wet hides for a trader for whom the applicant proposed to carry wet hides, was evidence that suitable transport facilities in the district intended to be served by the applicant were in excess of requirements."[3]

It is also clear that in cases where evidence is submitted proving the existence of idle vehicles in the district, then the grant of further tonnage would constitute the creation of excess facilities. For in a case in which it had been proved that " some of the motor vehicles which they [the existing operators] were authorised to use were standing idle through lack of work,"[4] the Chairman of the Tribunal expressed his agreement with the Traffic Commissioner who had said that the appellants had failed to prove that the grant of the application would not have " created an excess of haulage facilities in the district." And he added, referring to evidence the substance of which had been to the effect that there were already vehicles lying idle : " even if it could be said that the appellant established a *prima facie* case the evidence in rebuttal, both with regard to the facilities for carrying general goods and with regard to the facilities for carrying livestock, was overwhelming."[1] It was clear that the existence of idle tonnage belonging to those already operating in the district was held to be a determining fact in deciding the case.

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[1] It may well be asked how there can be any traffic carried by a " newcomer." The answer lies in the fact that a number of carriers came into the business after the expiration of the " basic year," which ended April 1st, 1933, so that, although they had operated vehicles for some time, they appeared before the Traffic Commissioners as " newcomers."

[2] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol. 23, p. 276.

[3] *Ibid.*, Vol. 23, p. 276.

[4] *Ibid.*, Vol. 23, p. 197.

It now remains to review the actual circumstances in which newcomers have, in fact, been allowed to enter the industry. Although these circumstances follow logically from what has already been said, it would seem that they are worth recording, not only because certain additional (though minor) points of principle emerge therefrom, but also lest an impression be left that entry to the industry has been entirely brought to a standstill.

As has already been shown, one of the outstanding conditions that must be fulfilled in the granting of licences to newcomers is that the traffic must be "new" traffic.[2] It follows from this that if it can be proved that there has been a "general" expansion of business in the area to be served, additional tonnage may be granted. A special significance attaches to the word "general"; for it would not necessarily be sufficient to prove that there had been an expansion in the business of a single firm. For in that case a "contract" licence might suffice. Nor would it be sufficient to prove the expansion of the business of a single haulier, for in that case the expansion might be due to obtaining traffic from other hauliers, and would therefore be abstracted traffic. If, however, there is evidence of a general expansion of (say) the building trade in a particular area, then a grant may be made. Thus, in Fry's appeal the Chairman of the Tribunal said: "The conclusions of fact which we have come to after hearing the evidence of Mr. B. are that in the district of Appledore, Northam and Westward Ho an increasing amount of building development is taking place, and that the facilities provided by the existing operators are not sufficient to meet the present needs of the building and allied trades . . . . Having come to these conclusions it remains for us to apply the principles outlined in our decisions on Ridgewell's and Enston's appeals, and, applying these principles, we allow the appeal."[3]

A second circumstance in which newcomers have obtained licences has been when the Tribunal has been satisfied that some "new type" of service is proposed to be given. This circumstance may be illustrated both by reference to Hughes' and Smith's appeals. In Hughes' appeal "the appellant was a smallholder of Pickworth in Lincolnshire. On July 27th, 1934, he bought a lorry, specially adapted for carrying cattle. On July 30th, 1934, he applied to Mr. Stirk, the Commissioner for the East Midland Area, for an 'A' licence."[1] The application was refused. An appeal was made to the Tribunal, and the decision of the Commissioner was reversed. In referring to the case, the Chairman of the Tribunal said:

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol. 23, p. 199.

[2] Provided also, of course, that there must not be other operators able and willing to carry out the proposed work.

[3] *Ibid.*, Vol. 22, p. 31.

"Three farmers were called before us whose evidence proved conclusively that there were no transport services of the nature of those proposed to be provided by the appellant available within a reasonable distance of many of the farmers in Pickworth and the district. One farmer proved that he had had to hire a lorry from Lincoln, twenty-nine miles away, in order to take his cattle to Grantham market. In our opinion, the appellant has made out his ground of appeal 'that the effect of the refusal to grant the licence is that there are not means of conveying large livestock within an area of over eighty square miles.'"[2] And then later: ". . . his evidence established a case that there were no other operators in the Pickworth district who provided the facilities that the appellant proposed to provide."[3]

A licence was also granted to a newcomer in Smith's appeal, in which it was held by the Tribunal that:—

> "An applicant who proposes to provide a new type of service, such as a regular daily service, is not bound to satisfy the Licensing Authority that the haulage he proposes to do cannot for some reason be done by other carriers already engaged in carriage, whether by road or rail."[4]

The applicant, "having been in the cotton industry, thought there was an opening for a regular daily service between towns in the Manchester district . . . One advantage claimed for this service was that it enabled those who made use of it to make up their cotton yarn or other cotton goods in small parcels and to have them collected and carried from day to day without having to wait until they had a sufficient quantity to be carried to justify them in hiring a lorry."[1]

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol. 22, p. 142.

[2] *Ibid.*, Vol. 22, p. 145.

[3] *Ibid.*, Vol. 22, p. 146.

[4] *Ibid.*, Vol. 22, p. 262.

The Traffic Commissioner took the view that "the appellant did not submit any evidence that the carriage of goods undertaken by him was of a specialised type for which existing facilities might be unsuitable."[2] But the Chairman of the Appeal Tribunal did not agree with this view. On the contrary, he said:

> "In our opinion there was evidence that the facilities provided and to be provided were of a special type such as did not exist in the Manchester district before the appellant started his service. We also are of opinion that the evidence showed that this regular service has been, and will be, of considerable advantage to traders in the district."[3]

Withal, there may be "special circumstances" in which licences may be granted, the Tribunal admitting that "many points have to be considered," namely "adequacy in numbers of vehicles, suitability of vehicles and efficiency of operation." A case in which "special circumstances" justifying a grant have obtained, has already been quoted.[4]

To sum up the positive conditions which in fact have permitted the grant of licences to newcomers. First, where a general expansion of industry is proved, and where existing operators are not fully meeting the needs of the district concerned. Second, where it is proved that a new type of service is to be rendered, hitherto not given by existing operators. Thirdly, where "special circumstances" exist, *i.e.*, when in the absence of the grant there would not be vehicles "adequate in quantity and suitably and efficiently operated."

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An attempt will now be made to recapitulate and summarise the new conditions in which new capital and industry may enter the industry in question.

First, a man is no longer permitted simply to "try his hand" in the haulage business. If he happens to be unemployed and/or the fortunate beneficiary of a relative's will, or if he is without capital himself but is prepared to purchase a vehicle on hire-purchase from a vehicle manufacturer, the right to provide facilities in advance of requirements is, for the time being at any rate, ruled out.

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[1] *Traffic Cases decided under the Road and Rail Traffic Act*, edited by Maxwell and Fay, Vol. 22, pp. 263–4.

[2] *Ibid.*, Vol. 22, p. 265.

[3] *Ibid.*, Vol. 22, p. 265.

[4] The service, owing to his special experience, could be more efficiently operated by the appellant than by others.

Next, even if he succeeds in proving that there are customers ready and willing to employ him, that in itself by no means ensures him a licence; and this, not because of any doubt on the part of the Licensing Authority that the customers will not in fact carry out their intention. As has already been seen, there have been " newcomers " who had, since the basic year, successfully established themselves in the business, but were not necessarily deemed worthy of holding a licence and continuing their business. They must go further, and make out to the satisfaction of the Licensing Authority that " the work cannot for some reason be done by operators," which means that it must be proved that those requiring transport had had, or in the absence of the grant would have had *difficulty* in obtaining the facilities required; and if in the opinion of the Licensing Authority (as opposed to the buyer of transport) existing services are " at least as good as " or equal to those proposed, then the newcomer would have to give way. Moreover, the fact that the traffic in question has at some previous date been carried by some other transport facility, may be taken as evidence that there has not been difficulty, that the service is at least as good, and that there is therefore no need for a new service. Thus, there must be a *proved and defined scarcity of or need for* the proposed service before a newcomer is permitted to ply for hire. The fact that the one service is, in the customer's estimation better and/or cheaper than the other, or for some reason is preferred to any other, is not a sufficient reason for a grant.

The Licensing Authority, whilst he must satisfy himself that facilities, apart from the grant of a licence to a newcomer, are " adequate in quantity and suitably and efficiently operated," nevertheless assumes the responsibility of judging whether facilities in question are of such a character or not. And the proved fact that a newcomer has been employed by traders for some time does not prove that the facilities available, apart from him, are not adequate, suitable and efficient. And in face of affirmed mutual advantage, as between traders and newcomers, applications for licences by the latter have been refused. In short, traders no longer have the right to exercise their own judgment as to which form of transport suits their particular requirements best. Their judgment may be, and has been, overruled by the Authorities in question.

Not that applications for licences on the part of newcomers have in all cases been refused. But if the application is to succeed the evidence put forward must be such as to prove either the virtual non-existence of the service proposed to be given, or at least some glaring inadequacy in the services already being given. Evidence merely proving that in the opinion of the trader the newcomer's service is best, whereas others are second best, is not deemed adequate to the proving of a case for a grant. A licence may also be granted if the traffic proposed to be carried is " new " traffic, due (say) to some general expansion of trade, and therefore traffic that has not been " abstracted " from other transport undertakings. But if the traffic that has been, or is proposed to be carried is " abstracted " from other transport undertakings, then facilities have been deemed to be " in excess of requirements," a circumstance the existence of which the Act was intended to prevent.

Taxation of Savings: A Reply

By A. C. Pigou

My dilatoriness in reading November's Economica makes this note, I am afraid, rather belated. But, if I may, I should like to comment briefly on Professor Macgregor's article upon "Taxation of Savings." For he makes Mill's analysis of that matter, and by implication mine also, depend on a somewhat notorious fallacy—the confusion between *source* and *object* of taxation—with which I cannot charge myself. My argument in "A Study of Public Finance" was directed to show that a general (permanent) income tax *differentiates against* savings in favour of expenditure. Thus in 1910 I spent £5 in buying an overcoat, which has done the State good service for the last quarter of a century. On the income of amenity received from that overcoat I have paid no tax. Had I spent the £5 in buying Consols, the income would have been taxed every year. Thus investment is hit in a way that expenditure is not. Had I spent the £5 on something immediately perishable instead of on an overcoat, the argument, though less snappy in form, would have been the same in substance. This argument does not in the least depend, as Professor Macgregor suggests, upon the assumption that a 10 per cent. tax on all income necessarily entails a 10 per cent. reduction of the part of income devoted to investment. As he rightly points out, that assumption is quite unwarranted—as unwarranted as the assumption that such a tax necessarily entails a 10 per cent. reduction in expenditure on a particular thing, say tobacco. I do not require, and did not make, any such assumption.

Two points may perhaps be added. First, the statement that a general (permanent) income tax differentiates against savings does not imply that it causes savings to be less than they would have been had there been no income tax. A situation is conceivable in which the supply of savings would be so inelastic that the differentiation has no effect at all on their amount. It is even conceivable that a general (permanent) income tax might cause savings to be *larger* than they would be if there were no tax; just as it is conceivable that a tax on diamonds might cause more diamonds to be bought. Secondly, the statement that a general income tax differentiates against savings does not imply that that portion of income which is saved *ought not* to be taxed. To that issue other considerations also are relevant, which it would be very improper to neglect.

Perhaps the point about differentiation is itself mainly a verbal one. What, with one set of postulates, is called differentiating against savings might, with another set, be called not differentiating in favour of them. My present purpose is not to argue about that, but merely to disclaim the charge of confusing "the basis of a tax and its source." I really do not believe, nor can I think Mill did, that a tax assessed on beer is necessarily "paid out of" beer.

Taxation of Savings: A Rejoinder

By D. H. Macgregor

I take expenditure, for the purposes of this argument, to relate to real income obtained within the year. When a durable article, like a piano or Professor Pigou's coat, is bought, it is a fiscal accident that the continuing income from it is not taxed in all cases. This accidental consideration should be kept out of the question. Second, I pointed out that the effect of income taxation might be to increase private savings. And third, as regards Professor Pigou's last remark, it is a tax on income that is in question, not a tax on savings, so that it was easier to assume about large divisions of income what would not have been assumed about a particular commodity.

Mill said that, after a tax of three per cent. on income, a further three per cent. was laid on "the remaining 97" of savings. This seems to me clear. Einaudi also seems quite clear when he says (*Corso*, p. 183) that a ten per cent. tax on an income of £10,000, of which half is saved, reduces savings to £4,500, and Ricci answers him on the same assumption.

Professor Pigou says (*Public Finance*, p. 136) that a tax of x per cent. *strikes* the expenditure at this rate; and that, "if £100 is put away for saving, it *removes* x pounds from it at the moment," so that "£$(100 - x)$ are actually turned into the investment." As my interpretation of this formula is not what he means, I think that a natural misconception should be guarded against.

I take this argument to be concerned solely with the question whether there is actually a fiscal differentiation against savings. There is an abstract assumption that the costs of government, which make income possible, are proportional to the income which accrues. I think that, in the legal sense, there is no differentiation; and that, in the social sense, the tax differentiates if it causes savings to be reduced in relation to expenditure, when the application of the tax is included. In the personal sense, the question is whether there is a greater charge on the taxpayer, in respect of his actual savings, for the same amount of protection, than in respect of his expenditure as above defined. I agree that there are other considerations, and definitions of income, which would raise further problems. Some of these; which Professor Pigou mentions in his book, were in issue between Mill and Gladstone in the discussion referred to in my paper.

Farmers and Income Tax

By R. S. Edwards

Many economists are giving considerable attention to the cost of keeping alive, in this country, certain forms of agricultural production. It seems, therefore, that the time is opportune for a closer examination of that curious form of subsidy, the Income Tax provisions which place the farming community in a very privileged position by comparison with those engaged in other types of business. In order to discuss the scope of this subsidy, it will be necessary to examine briefly, firstly the rules relating to the taxation of trades and professions in general and then the special rules applying to husbandry.

For the purpose of taxation, incomes are classified into types, described and limited in scope in the five Schedules to the Income Tax Act, 1918. Most businesses fall for assessment under Cases 1 and 2 of Schedule D, and except for the years in which the business is commenced and wound up, the assessment is made each year on the profits of the accounting year ending within the Income Tax year preceding the year of assessment. For example, if a trader's Profit and Loss Account for the year to 31st December, 1935, shows a profit (adjusted for Income Tax purposes) of £500, he will be assessed on this sum for the Income Tax year 1936–37 (*i.e.*, the year to 5th April, 1937). Thus rises and falls in profit are reflected in tax assessed a year later. Now in the case of income arising from the occupation of land, of which farming is one form, the rules of Schedule B apply—rules very different from those of Schedule D. It should be noted that the Act draws a distinction between occupation of land solely or mainly for the purposes of husbandry and occupation for other purposes, *e.g.*, as pleasure grounds. We are here concerned only with the rules relating to husbandry. There is no statutory definition of " husbandry" ; it would seem from the decided cases, that it is not confined to tillage or cultivation, but includes grazing and rearing of sheep and cattle, the production of milk, and poultry farming.

Schedule B provides that the profits of farmers derived from the occupation of land are not normally to be computed for Income Tax purposes by preparing a Profit and Loss Account and ascertaining the actual results of the year. Instead it is laid down that the assessment is to be made on a conventional figure which is re-examined once every five years and is known as the "annual value." This "annual value" is to be arrived at by application of the same principles which apply to valuations for Schedule A; that is to say annual value is the rent at which one might reasonably expect to let the property year by year. It is believed that the idea of this rough rule-of-thumb method of assessing tax arose in the first place because of the difficulty of ascertaining profit more accurately. For example, it is alleged that it is difficult to make farmers keep books and account for the value of produce which they consume themselves or pass on to their friends. In the words of the Royal Commission on Income Tax, 1920, the theory underlying Schedule B "was that there was some regular correlation between the rental value of the land and the farmer's profits."[1] From 1896 to 1915 one-third of the annual value was taken as the measure of profit; in 1915 there was an increase to the full annual value. From 1918 to 1922 this was doubled, but then reverted to the annual value again.

A simple example will perhaps make the present position clear.

X lets a farm to Y at an annual rent of £500 out of which X has to pay tithes £35, land tax £5, and repairs to the property. Tax would become due on the following assessments: *Schedule A* (tax on income from ownership).

Gross annual value		£500
Less tithe	£35	
land tax	5	
repairs (statutory relief ⅛.)	63	
		103
Net annual value which is assessed as *unearned* income of X		£397

Schedule B (tax on income from occupation)

Assessable as *earned* income of Y

Gross Annual value	£500

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[1] This assumes that the application of the labour and capital of the occupier is equally intensive in all cases.

Thus Y pays tax, not on the profit he makes, but on this figure.

Such a method of assessing liability to Income Tax would, if there were no modifying provisions in the Act, have the advantage of costing the State little in computation and collection of tax. Moreover, if it could be shown that, by and large, the profits of farmers are roughly equal to the rents they pay, then it could not be maintained that farmers as a class contribute less to Income Tax than those engaged in other occupations and earning similar incomes. All that one could say is that the method lacks precision and that, as the valuation is fixed for five years, actual fluctuations in profit would not be reflected in income tax liability.

However, there are important modifying provisions which give the farmer alternative methods of assessment which place him in a very advantageous position. These provisions are found in Rules 5 and 6 of Schedule B. Rule 5 provides that anybody who would be assessed to tax on this conventional basis " may elect to be assessed and charged under Schedule D," that is to say, on the actual profits of the accounting year *preceding* the year of assessment as described earlier. Thus suppose, in the example given above, Y or his accountant prepared accounts for the year to 5th April, 1935, and they showed a profit of only £200, then he could claim, by giving notice on or before 5th June, 1935, to be assessed on this figure for the tax year 1935–36 instead of paying tax on £500. Now suppose the accounts for the year to 5th April, 1936, showed a profit of £1,000, then for 1936–37 the farmer would be assessed under Schedule B on £500. It will be seen later that the importance of the Rule 5 claim lies in the fact that the profits to be computed under Schedule D are those of the preceding year and not those of the current year.

Rule 6 provides that if for the actual year of assessment the farmer can show that his profits are less than the conventional figure of annual value, he can claim a reduction of the assessment to the actual profits and if the result for the year is a loss he can set this loss against other income of that year.

It will be clear, therefore, that the farmer has three alternatives from which to choose in any one year; and election made in one year does not preclude a different election the next year. He can choose to be assessed on the annual value or the profits of the current year or the profits of the preceding year. Suppose then Y made no profit at all during the year to 5th April, 1937, then his assessment for that year would be reduced to nil, but what is more, for 1937–38 he would claim to be assessed under Schedule D on the profit *of the preceding year*, that is to say on nothing, although let us assume he makes £500 for the year to 5th April, 1938. The position for the years 1934–35 to 1937–38 would be as follows:

	Actual Profit.	*Assessments.* (after adjustment)
	£	£
1934–35	200	200
1935–36	1000	200
1936–37	—	—
1937–38	500	—
	£1700	£400

The illustrations set out below shew the assessments which would be raised on a farmer and those which would be raised on any other business making the same profits and losses. Those who are familiar with farming profits will agree that the figures are not exceptional.

EXAMPLE I.

Schedule "B" Annual Value £170.

Profit or Loss adjusted for Income Tax purposes.

	£
Year 1	+ 90
„ 2	− 240
„ 3	+800
„ 4	+800
„ 5	+150

	Assessments on any other type of Business.		Assessments on farmer.	
		£	£	
Year 2	90			
Less Section 34 claim	90			
		0	0	(Rule 6)
Year 3		0	0	(Rule 5)
Year 4	800			
Less Section 33 claim	150			
		650	170	(Sch. B.)
Year 5		800	150	(Rule 6)
Year 6		150	150	(Rule 5)
		£1600	£470	

Note: In addition a claim could be made to set the loss of £240 against other income in the year of loss.

EXAMPLE 2.

Schedule "B" Annual Value £240.

Profit or Loss adjusted for Income Tax purposes:

	£
Year 1	+ 20
" 2	− 300
" 3	− 400
" 4	+1550
" 5	+ 800

	Assessments on any other type of business.		Assessments on farmer.	
	£	£	£	
Year 2	20			
Less Section 34 claim	20			
		0	0	(Rule 6)

	Assessments on any other type of business.		*Assessments on farmer.*	
	£	£	£	
Year 3		0 ..	0	(Rule 6)
Year 4		0 ..	0	(Rule 5)
Year 5	1550			
Less Section 33 claim	680			
		870 ..	240	(Sch. B.)
Year 6		800 ..	240	(Sch. B.)
		£1670 ..	£480	

The losses of £700 could be set against other income of the years in which the losses were incurred.

The important points are that farmers need never pay tax on more than they earn but they can very frequently pay tax on less; the "annual value" operates as an upper limit only. Moreover a bad year can be made to do service *twice* for income tax purposes. The argument that farmers are not sufficiently acquainted with book-keeping to keep the records necessary to provide the required information will not bear examination. Standard forms of analysed Cash Books have been prepared by accountants and little instruction is required in keeping them; for a few guineas a year the professional accountant will do the rest. Secondly it is not difficult to assess the value of produce consumed by the farmer's family. Inspectors of Taxes, from a comparison of large numbers of cases, can get at the facts fairly accurately.

The Royal Commission on Income Tax 1920 criticised the Schedule B regulations. In the words of the Report: "If farmers at the present day do not keep books we think such a state of affairs is to be deplored, but we see no reason why specially favourable Income Tax treatment should be allowed to them as a consequence of that neglect." The Commission recommended that farmers should be transferred to Schedule D, but nothing was done about it. Incidentally, the Report stated that the option under Rule 5 was rarely exercised. This was due to the fact that the three years moving average method of assessment—which no longer exists—off-set the tax advantages of a bad year. It is now possible to get the tax benefit from a poor year without it being possibly swamped by two better ones, and many farmers are taking full advantage of this fact.

If it be accepted that farming is subsidised through these Income Tax provisions it is worth noticing that the extent of the subsidy cannot be calculated because farmers send to the Inspector of Taxes, Profit and Loss Accounts only for those years in which profits fall short of the Schedule "B" valuation; that is to say, no information is available to the Revenue as to profits of farms in any years in which these profits are greater than the annual value. The subsidy does not vary with the volume of output of particular commodities; it depends partly on fluctuations in profit and to this extent is an advantage to farmers who produce those commodities the prices of which are relatively unstable. Secondly, it tends to increase more rapidly than the scale of income because income tax and surtax are progressive taxes. Thirdly, it increases in years of high rates of income tax and surtax. Fourthly, it varies with the amount of other income which the farmer has. Lastly, it is greater as more of other factors are "applied" to land. If the Government is desirous of subsidising the production of foodstuffs it should at least consider whether it wants the sort of subsidy described above. The law already insists that profits arising from "land occupied as nurseries or gardens for the sale of the produce" shall be taxed on the actual profits earned in the previous year, so that this type of agriculture receives no favoured treatment, and must prepare accounts as other businesses do.

Again, where lands are occupied by a dealer in cattle or a dealer in, or seller of, milk and the Commissioners find them *insufficient for the keep of the cattle* brought on to them, and they find that the annual value does not afford a just estimate of the profits of the business in cattle or milk as the case may be, they may charge the occupier under Schedule "D."

It is suggested that the following changes might well be made:—

1. All farms of an annual value of £150 and over shall be assessed according to the rules of Schedule "D." The

annual value of farms under the same control shall be aggregated for this purpose.

2. If the occupier of land of the annual value of less than £150 desires to be assessed under Schedule " D," he shall be allowed to do so only if he agrees to be assessed in this manner for all future years of occupation of the property. This provision already applies to woodlands managed on a commercial basis.

3. The right to have the Schedule " B " assessment reduced to the actual profits shall be withdrawn in order to prevent farmers claiming this relief in bad years and paying only on the conventional basis in good years. If a farmer can produce accounts for years when he is doing badly, then there is no good reason why they should not be produced when times are better.

Book Reviews

Soviet Communism: A New Civilisation? By SIDNEY and BEATRICE WEBB. Longmans, Green & Co., Ltd. 1935. 2 vols. xix + 1173 pp. 35s.

I

This, the latest of the Webbs' great books, is also the most characteristic one, for it demonstrates more clearly than any of their other productions the value and the limitations of their method. Like their history of English local government the present book is a masterpiece of arrangement, and a stirring parade, almost a pageant, of drilled evidence. Such is their command over detail that no critic, however partial, will find a single important fact missing or misplaced. And even where he happens to disagree with the authors' interpretation, he will have to acknowledge the same accuracy and clarity of statement which has made the Webbs the true, and perhaps the only, social encyclopedists of our age.

The admirers of the Webbs, and the reviewer among them, will also be impressed by the other quality of the book—its social purpose. For all their love of Russia and admiration for the communists, it is for England, and on behalf of the Fabian ideal, that they have written their book. This preoccupation ennobles most of their work, but in no other of their major productions does it shine with such a clear and insistent light.

These characteristic qualities have not been imported free from the equally characteristic defects of the Webbs' work, but strange as it may seem, the reviewer's main criticism is that one at least of these defects is not here present in the same generous measure as in the other books. What critics have come to regard as the most typical and easily recognisable of the authors' limitations is their special attraction for the machinery of government. They constantly refer to themselves as political scientists, but they would probably have done themselves better justice if they had chosen the name of constitutional morphologists. Their genius lies in their understanding of the methods of government and skilful use of official evidence. In spite of all temptation to the contrary they have fought shy of the problems and the methods of the sociologists who deal with the intangible substance of human relations and psychological states.

This feature will, in some measure, also be found in the present book. The bulk of the first volume and some of the second are devoted to the institutions and the constitutional arrangements of the Soviet state, and are based on official evidence. But conspicuous as this " morphological " bias is, one wishes there were more and not less of it. There is room for a well documented description of the constitutional forms, and even the constitutional fictions, of the Russian state, while the reliance on official evidence is welcome if only because it provides the reader with the first English version of the Soviet apologia-English in presentation as well as language.

Unfortunately this book attempts something more. Most of the second volume and some of the first deal with the substance of Soviet policy and its effects on Russian life. That for an enterprise of this kind the authors are seriously under-equipped will be obvious to every detached reader, and would have been obvious to the authors themselves had they not, in spite of all their wisdom and experience, undertaken their Russian journeys, which like some more recent German journeys, merely provide their victims with the oral and ocular substitutes for knowledge. Yet what vitiates these parts of the book most is not the authors' lack of equipment, and not even the illusion of understanding which their physical contact with Russia produced, but their selection and use of evidence. Here they have had to employ evidence of a kind they do not regularly employ: personal accounts, impressionistic studies, newspaper reports and episodes in novels and short stories. This alone should have made them doubly careful in the selection of their authorities, and doubly critical in their use. How strange it is, therefore, to find the authors assembling a scratch team of suspect witnesses and treating it with all the recklessness of youthful pleaders!

Thus the person whose accounts and interpretations are quoted most often is the dispassionate Mrs. Anna Louise Strong, and two out of every three accounts of the routine of Soviet life are derived from the feuilletons of that notably independent periodical the *Moscow Daily News.* The literary output of young Anglo-Saxon travellers is generously drawn upon, and even official propaganda pamphlets are not spurned. It is not that the authors are ignorant of the witnesses for the prosecution, for nothing can be more catholic and impartial than the footnote bibliographies by which each new subject is introduced. But very little of this catholicity has been allowed to penetrate into the text. Hoover and Knickerbocker are quoted very seldom, Chamberlain is cited mostly where he happens to approve, or where the authors wish to refute him, but hardly ever as a witness for the main trends of Soviet policy. Of all the accounts of Russia written by Englishmen who lived in Russia, both before and after the Revolution, the one which receives most attention and the highest commendation is that of the notorious Dillon,[1] while the only émigré politician to be quoted at length is Mazepa, whose article happens to lend unintentional support to the anti-peasant case. Other and similar instances could be cited by the score.

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[1] Dillon was once before " converted " by a Russian Government, and his despatches about the virtues of the Czarist government earned for him the reputation of being Witte's man, which, however, did not cure him of his propensity for being " converted " to Government causes in other parts of the world.

II

These peculiarities of method were bound to produce a book of great but very unequal excellence. Its uncontroversial parts are probably better than anything ever written about Russia, its controversial parts will, let us hope, soon be forgotten. The former are more numerous than would at first sight appear, and are not necessarily confined to the wholly uncontroversial description of institutions. Thus most of the historical references to pre-revolutionary Russia and the period of war communism are both accurate and convincing. Few readers will quarrel with the account of the part which the communist party played in the development of the revolution, or still plays in the Soviet state. Even in the sections dealing with Soviet policies and achievements there is a great deal of what no sane critic will dare to deny. There is no denying the remarkable progress of industrialisation, or the great effort of the Soviets, especially in the periods preceding and following the first five-years plan, in the fields of education and public health. Most observers of modern Russia will also acknowledge the liberating effect of the Soviet régime on the smaller and more backward nationalities (*i.e.*, other than those of Georgia and the Ukraine). For his own part the reviewer is prepared to accept the contention that the communist revolution was carried out in order to establish a classless society, and that the Soviet policy has in fact abolished most of the capitalist class distinctions. He will also accept the evidence of the authors, and of others who have investigated the problem more recently, showing that the factory workers are both happier and better off under the Soviet rule than they were throughout the Czarist era. Some people, and the reviewer among them, would go even further than the authors in their acknowledgment of the formidable military strength which the Russian government has built up in recent years. All these are immense achievements fully meriting the eulogy which the authors bestow upon them.

The book also contains a great deal of controversial matter, which should not and will not be taken too seriously. There is no danger of anybody being misled by the argument that Stalin is not a dictator. We are told that there is no provision for the office of dictator in the Soviet constitution, as if this proves anything else than the danger of using the Soviet constitution as evidence of political realities. We are also told that the dictionaries derive the term "dictator" from the verb "dictate," and that Stalin, when being observed by Mrs. Anna Louise Strong, consulted others before arriving at a decision—as if Hitler never takes advice, and Mussolini transacts all his business by means of parade-ground orders.

But the book also contains highly controversial statements which may take in the unwary. Some of them occur in the sections dealing with recent Soviet history and the present Soviet policy, and especially the story of Stalin's rise to power and of the struggle over the alternative versions of the five-years plan. Even a cursory reference to the *Planovoie Hoziaistvo* of 1928, and 1929 would have shown the authors how irrelevant are some of the arguments which they employ and how unjust they are to Groman, Bazarov, Osadchi and the rest of the Gosplan staff. Economists will be especially sorry for the very cursory treatment of the economic issue which that dispute raised and which now agitates the socialist economists in western Europe, *i.e.*, the problem of the price mechanism in a planned economy. But what will surprise the admirers of the Webbs most are the arguments to which they stoop in their endeavours to justify the collectivisation of agriculture. In order to account for the ruthless methods of the Soviets a picture of peasant life is painted in which the peasant is made to appear as a mixture of a savage and a bandit who deserved the punishment he got.[1] It is somewhat disturbing to find that in trying to estimate the immediate consequences of the policy the authors condescend to an etymological quibble about the real meaning of the word famine, but it is even more disturbing to find the continually recurring statement that the kulaks and the rebellious villages were exiled to Siberia and the Arctic with the humanitarian intention of saving them from their self-inflicted starvation.

Indeed, the whole picture of the peasant revolt is made to fit into its official version by continuous recourse to the undistributed middle. From stray references and from illustrations drawn from all over Russia, including Kuban, Turkestan and Maurice Hindus' homeland (which is presumably in the Western provinces) we gather that during the struggle for collectivisation " whole villages abstained from sowing " ; that what the Government had to face was in fact " a general strike of the peasants " (p. 265) ; that in order to cope with it whole villages had to be removed. And yet when it comes to a general summary, an impression is conveyed that the transformation was achieved at the cost of " driving out the universally hated kulaks and the recalcitrant Ukrainians and Don Cossacs," while the whole process is described as the " liquidation of the kulaks " (pp. 561–573) and introduced by long passages from Dr. Dillon and others showing " the abhorrence and hatred felt by the Russian for the kulak."

There is a similar ambiguity—let us hope an unintentional one—

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[1] Several pages (pp. 808–811) are devoted to a long quotation from Dillon containing his famous description of the peasant as " half a child and half an imperfectly tamed wild beast." In another place (pp. 238–239) the authors suggest that prosperity, even if attainable, would have been wasted on the peasant. For did not the return of prosperity under the N.E.P. merely result " in the marked development in the villages of the characteristic peasant vices of greed and cunning varied by outbursts of drunkenness and recurrent periods of sloth " ?

in the definition of the agricultural crisis which induced the collectivisation. The authors are doubtless right in assuming that the " scissors," *i.e.*, the disparity between the industrial and agricultural prices, raised great difficulties calling for government action. But in asserting this, they also let drop a suggestion that a similar disparity was at that time experienced abroad. Are we then to understand that the disparity of the " scissors " was of the same magnitude, and due to the same causes, as the low prices for raw materials in post-War Europe? It is a pity that the authors have not provided the economic sections of the book with the necessary statistics and have not discussed the economic issues in sufficient detail. Had they done so they could not have helped bringing out the fact that the divergence between industrial and agricultural prices in Russia was many times that in the West, since grain was taken from the peasants at much below the world prices, whereas industrial products were sold to them at many times their value abroad. They might then even have realised that the true cause of that purely Russian phenomenon was the fact that the urban workers and the bureaucracy were earning more than the true value of their product, and were little more than expensive pensionaries of society. And if that was so, the way out of the scissors—less painful if only because it would have involved fewer victims—would have been to stop the policy of protecting the costly proletariate at the expense of the peasants. In order to do that they would have had to assume that the factory workers and the peasants were both human beings with equal claims on society and on the sympathy of enlightened foreigners, and this would perhaps have been as outrageous an assumption for the formula-bound and machine-mad Marxists as once upon a time it was to Ure and the other English apostles of the Industrial revolution. But is it too impossible an assumption for the authors of the *English Poor Law History?*

Even more controversial is their general view of Soviet life. Few students of Soviet Russia, and even few Communists, will be prepared to accept the picture of spontaneous and voluntary democracy pervading every form of public activity. If the description of the communist party as the organisation representing the vocation of leadership is both subtle and true, the impression it conveys of a leadership voluntarily accepted by the rest of society is merely simple. One of the most impressive things about the communist rank and file has been the hard life which many of them, a feared and hated minority, led in the villages and towns of Russia throughout the first ten or twelve years of the Revolution. It is in order to impose and to defend their leadership that the machinery of the Cheka and the G.P.U. was originally designed, and it is because the leadership was an imposed one that the ordinary democratic guarantees had to be revoked.

Equally simple is the discussion of the motives underlying the communist leadership. To deny the acts of devotion and self-sacrifice which fill the history of the civil war and the five-years plan would be both ungenerous and wrong. But it would perhaps be too generous and equally wrong to underestimate the action of other and baser motives. Even before the Soviet law permitted the communists to earn the highest salaries in the state, it had raised them to the highest positions of power in society. If Russia for a time abolished the inequalities of income, she also created inequalities in the direct distribution of power the like of which the modern world has never seen. The result has been to provide the party man with a bundle of motives of which the psychological roots are buried far deeper and are established much more securely, than those which the authors emphasise.

This distribution of power within the society and within the party agrees badly with the show of democratic opinion which captivated the authors. They seem to have discovered in Russia a spirit of public criticism more active than in almost any other country, expressing itself in that continuous self-castigation which fills the columns of the Soviet press. What a pity they read the *Moscow Daily News* and not the *Pravda*. Had they read the *Pravda* they would not have misunderstood the true function of self-criticism in the Soviet State, which is not that of public control, but that of government propaganda. Gide describes it as " criticism to order " for it proceeds in carefully managed waves or " campaigns." Each campaign usually starts with a public speech by an outstanding party leader, or more commonly with a series of such speeches and a suitable resolution at a conference ; it is then followed up by a fulminating article in the *Pravda* or the *Izvestia*, which in its turn is immediately succeeded by a flood of denunciatory letters, articles and resolutions from all over the Union.

It is this marionette quality of public responses that, in the opinion of the reviewer, constitutes the true indictment of life under the Soviets. Persons with intimate friends among Russian communists have amusing tales to tell about the Soviet quality of not only acting but also of thinking to order, or what is still more important, of suspending all judgment until the order has been given out. Mere artists like Gide, Céline and poor Eugène Dabit, found this goose-stepping of thought and conversation sufficiently terrifying to colour their whole view of the country ; and even the Webbs are not completely unaware of what they describe as the " disease of orthodoxy " or of the activities of the G.P.U. and the Commissariat of Interior. What they seem to be unaware of is the extent to which orthodoxy and the police rule are interconnected, and above all the extent to which both are essential parts of the Soviet régime. To them, both orthodoxy and the G.P.U. appear as passing afflictions of the age of transition, something which does not belong to and does not affect the core of Soviet life. Passing afflictions ?—But where are the signs of their passing ? In spite of the democratic constitution which Stalin has inaugurated since the writing of this book, and in spite of his attempts to reconcile the non-communist elements abroad, the field of orthodoxy has lately been widening and its rules tightening. Whereas in the first ten years of the régime the communists themselves were allowed a certain freedom of discussion within the limits of the fundamental party principles,[1] Communist opinion at the present moment is being regimented, if anything more strictly than that of outsiders. Since Trotsky's first defeat over his demand for free discussion within the party (to which, by the way, the Webbs hardly refer) the heresy hunt has been getting more and more ruthless, with the result that, at present, freedom of debate even on problems far removed from the fundamentals of the Soviet creed is confined to the few stalwarts of the régime.

This process should not be, though it commonly is, regarded as an accidental or unimportant by-product of the Soviets. A totalitarian régime has its logic, which can and does lead the ruling clique to the progressive narrowing of its political base. English students of Russia are apt to forget that the original programme of the October revolution demanded the union of all the socialist parties and advocated the exclusion of the bourgeois liberals only. But the same reasons which prompted and justified the exclusion of the liberals, also prompted and justified the exclusion, in successive instalments, of the right Menshiviks, the right Socialist Revolutionaries, the Menshiviks of Martov's group, the left Social Revolutionaries, the first left deviationists, the first Trotsky opposition, the right deviationists, the second Trotsky opposition. A totalitarian state in the hands of totalitarians is not a chariot but an avalanche; it is driven by its own momentum, and sometimes buries the very men who started it in the hope of a short drive.

Least of all should this feature of the régime be regarded as irrelevant to our final judgment of the Soviet system. To the reviewer—and here he will allow himself to be rabidly and unashamedly partisan—the creation of a subservient parrot-man vitiates the whole communist achievement. There is no need in this country to remind people that the whole case for socialism is ethical. None of the economic problems of the capitalist system are incapable of solution within the system itself, and as long as it can maintain its unemployed even the problem of crises and unemployment is essentially moral and psychological. In relation to the real objects of socialism public ownership of capital and control of production are merely tools, and the true nature of a system is to be judged not by the economic tools it has adopted and not by the phraseology it employs but by the human ends it has attained—the men and the life it creates.

M. POSTAN.

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[1] It is a great pity that in their references to the "open discussion" of policies in party councils the authors either do not mention, or do not pay any attention to, dates. A chronological table of issues which were discussed would be very revealing!

Socialism—An Economic and Sociological Analysis. By LUDWIG VON MISES. Translated by J. Kahane. London: Jonathan Cape. 1936. 528 pp. 18s.

Professor von Mises published *Die Gemeinwirtschaft* originally in 1922, and the second German edition in 1932. In this translation certain additions, not to be found in that second edition, have been inserted, notably upon the problem of calculation and on unemployment. Although in one sense he writes *sub specie æternitatis*, yet it cannot be denied that time has staled his infinite variety, and that, with certain exceptions, the work seems inevitably dated. And 1936 is a long way from 1922, with more practical experiments in authoritarian and totalitarian forms of socialism and planning in capitalist societies than the author, when he laid out his ground plan, could have thought possible. For extensive treatment is given to writers and their notions that have passed into the limbo—over generous space given for museum pieces, when there is a menagerie to be caught and labelled.

In this work of well over 200,000 words, the canvas is vast, but not crowded, though crowds are upon it. To change the metaphor, we go a long way back into history, deep down into law, philosophy and ethics, wide afield for everything that savours of check upon pure competition and can be related to a socialistic concept, in this massive, erudite, encyclopædic work. Although it has been my companion for leisure moments for over a month, I have never felt lost, or bored, or wanting a sense of direction, though sometimes my companionship seemed a little unreal. The proportion of the work that can be called strictly economic, either analytically or inductively, is relatively small.

Part I, " Liberalism and Socialism," covers such subjects as ownership and ideas of property, the policy of contract, problems of married life, prostitution. Part II on the "Economics of a Socialist Community," deals with the organisation of production (which touches the key point of the author's whole attack), the distribution of incomes, the position of the individual, dynamic conditions for capital and demand, foreign relations. It also sketches temporal types—Guild socialism, Christian socialism, syndicalism, etc. The third part treats of the alleged inevitability of socialism—by evolution through various forms, by concentration of capital and fortunes, by monopoly. Part IV on " Socialism as a moral imperative," covers all the ethical systems and arguments, and Part V, entitled " Destructionism " covers all kinds of interferences with *laisser faire*, such as labour legislation, social insurance, trade unions, taxation. In many of these sections the treatment is conventional and neither novel, elaborate nor profound, but it is never merely superficial or uninformative, and it forms a definite part of the author's comprehensive plan, which in his fierce anti-socialist hunt, is " to leave no stone unturned and to explore every avenue," with such diligence as to have the conventional politician beaten in these activities. In many of the parts, however, there is erudite matter and acute observation not to be found elsewhere in the literature of the subject.

The economic student of 1937, rather than of 1922, turns to the chapters on economic calculation as the contribution of vital importance. Indeed, Professor von Mises himself must stand or fall by this part of his work, for he dismisses socialism in various forms quite summarily many times in other parts of the book, on the ground of the absence of the apparatus of calculation, quite bluntly stated as ending the matter. If he is wrong, and there actually *is* a substitute for price in practical socialism, or totalitarianism of any kind, then not merely this section, but most of the rest, must be reargued to justify the conclusions. " To prove that economic calculation would be impossible in the socialist community is to prove that socialism is impracticable." (p. 135). The reader turns then with some eagerness to the problem of economic calculation, and suffers perhaps some disappointment, unless he is acquainted with the history of the development of the subject. Of modern writers Professor N. G. Pierson came first in 1902 in placing stress upon the problem of value, but the idea was more definitely and clearly worked upon by Professor von Mises in an article in *Archiv fur Sozialwissenschaft und Socialpolitik*, April, 1920, and this has been recently made accessible to English readers in *Collectivist Economic Planning*. The same article naturally forms the backbone of his chapter on the subject in this large work on Socialism under review, published two years later. Apart from some engaging differences in emphasis in translation, therefore, we have substantially the same matter, with elaborations. But it does not actually carry us further than the original article, either in incorporating and commenting upon recent practical experiments and developments of artificial or forced pricing in semi-collectivist conditions under dictatorships, or in replying to the more significant attempts to evade his line of reasoning made by the most recent writers. Thus one short paragraph on Russia simply states that she has not had to face the problem squarely because she moves in a world of pricing economic systems and her own values are referable thereto; like state and municipal enterprises elsewhere which lean upon the price systems of individualism, in which they grow, for all their elements of calculation, Russia looks over the shoulder of the writer at the next desk and sneaks his results. " Without the basis for calculation which capitalism places at the disposal of socialism, in the shape of market prices, socialist enterprises would never be carried on." The Fascist and Nazi régimes contribute nothing to this discussion.

Professor von Mises' contribution to this subject goes further than his predecessors because he does not confine his stress to the value or price of final objects of economic consumption, but deals also with the price element in the mechanism of production, in intermediate products and the application of working capital. He is also concerned to find a calculus for the application of resources to the most eligible margins.

So far as the discussion of principle is concerned, he replies to two early Continental critics in an Appendix—treating those who rely on a purely Labour theory of value as not worth discussing—Polanji (1924) who, he claims, finally fortifies his own conclusions, and Heimann (1922) who works on a theory of costs, and, if costs mean a loss of utility, inevitably reasons in a circle. We may perhaps accept Professor Hayek's conclusion that the sum of continental discussion in the years following the publication of the work left the central thesis undamaged. But, for meeting the most recent British critics in the *Economic Journal*, and elsewhere, we shall not find any direct help, nor would it be reasonable to expect it, and the student is left to found, upon Professor Mises' excellent basis, his own analytical counterblasts. We do not belittle his pioneer work when we say that by to-day's standards it is not thorough enough to justify the immense weight of his conclusions.

This great work, judged by British standards, is over-elaborated in parts and much of it has already entered the museum stage. Readers with little leisure may think it tiresome in parts. It can be recommended to the harassed student rather as an encyclopædic work of reference (with its remarkably complete index) than as a model textbook for study. But it has a permanent place in the history of the development of thought; it is a full and photographic picture of the ideas of a particular time, and it has done yeoman service in giving a true measure of Marxism and influencing continental opinion saturated with Marxist ideas, for it lifted the ban, so skilfully laid by Marx, upon the examination of his scheme by the tests of scientific economic analysis. The sections which are perhaps most worth the English student's attention, in addition to that upon calculation and a " priceless " economy, are indeed those directly commenting upon Marx—to be followed easily through the index; those which treat of the fundamental problems of a socialist economy under conditions of change; those showing the all-importance of the concept of " labour time " in such an economy; those which show that assumptions of the moral purification of mankind are evasions of the problem, even if they cannot find a slick answer to every question that has arisen in recent years. Minds of every type of equipment will find material in the realm of fundamental thinking along many different lines, and Professor Mises' thought can be extrapolated into most of the problems of to-day.

J. C. STAMP.

Seasonal Variations in Employment. By CHRISTOPHER SAUNDERS. Longmans, Green & Co. 1936. xii + 311 pp. 15s. net.

Readjustment in Lancashire. By Members of the Economic Research Section, University of Manchester. Manchester University Press. 1936. 137 pp. 4s. 6d.

Mr. Saunders' study is one of the first attempts, on an adequate scale, to use the wealth of statistics produced in the administration of unemployment insurance, to throw light on a specific economic problem. In the first part of his book, the employment and unemployment statistics are analysed with a view to determining the regularity and range of seasonal variation of employment in each of the 100 industries of the Ministry of Labour classification of insured persons, and in all industries taken together. After a general chapter on causes of seasonal variation, the rest of the book deals descriptively and statistically with a few important industries, including motor manufacture, clothing, building, and agriculture.

The analysis of the first part begins by calculation for each industry of the number in employment at the end of each month; the numbers insured are obtained by simple interpolation from one July count of unemployment books to the next and deduction of the numbers unemployed leaves the monthly employment figure, which is then expressed as a percentage deviation from the twelve months moving average. Those industries—63 in number with nearly 77 per cent. of the insured persons—for which these percentages show each year a fluctuation of similar amplitude and pattern are classed as definitely seasonal; the extended median (*i.e.*, the central three or four) of the values shown for each month over the period 1924–32 is taken to present for that industry its normal seasonal variation, set out in graphs in Appendix III. The difference between the peak and the trough of the seasonal curve gives, in numbers and as percentages of the persons insured, the range of variation, *i.e.*, the numbers and percentages affected by the seasonal swing at its greatest.

The care with which the statistical analysis has been made is obvious from this brief description. Only on one or two points of detail is further explanation needed. It is not clear, for instance, how the dispute year 1926 or the change of scope in 1928, through exclusion of persons over 64, have been treated; nor is any notice taken of the distortion of statistics of female unemployment by insurance changes in 1930 and 1931. But these are small points which do not affect the main results or the great value of Mr. Saunders' analysis.

The first result is to show the generality of seasonal variations. Over and above the 63 industries classed as "definitely seasonal," 20 more with nearly 13 per cent. of the insured persons are attached to industries subject to some degree of seasonal fluctuation.

The range of fluctuation, *i.e.*, the difference between the numbers employed at the seasonal peak and the seasonal trough respectively, in the definitely seasonal industries runs from 12 per cent. or 13 per cent. of the insured in coal-mining, shipbuilding and cotton to 3 per cent. or 4 per cent. in distribution, road transport, printing and laundries. These percentages assume complete mobility of labour within each industry: since many of the industries are far from homogeneous, the actual numbers and percentages affected by seasonal fluctuation must be greater.

In combining the numbers for individual industries to get a figure for seasonal unemployment as a whole, Mr. Saunders has to make the opposite assumption—of complete immobility from one industry to another. On this assumption, from 600,000 to 850,000 or from 5 per cent. to 7 per cent. of the insured persons are thrown out of work at one time or other in the year through seasonal fluctuation. The numbers and percentages are higher in years of depression than in boom years, but form a smaller proportion of the total unemployment. Taking good and bad years together, the seasonal unemployment of the definitely seasonal industries may be put at 2 per cent. of the insured persons.

This, by post-war standards, is not a high figure, though allowance must be made also for industries which are "irregularly seasonal." Even so it seems clear that seasonal unemployment is less important in Britain than in Germany or the United States. Nor is there any general tendency for it to increase.

Seasonal unemployment cannot be presented and Mr. Saunders does not present it as one of the major social problems. The regularity of its incidence and its short duration make provision for it by insurance relatively adequate. It may still be asked whether there need be as much seasonal unemployment as there is. From this point of view Mr. Saunders' studies of particular industries are as important as his general analysis. The development, for instance, in the modern, highly protected and highly prosperous industry of motor manufacture of extremely violent seasonal fluctuation and a high level of total unemployment are phenomena discouraging to hopes of better social management of industry.

Readjustment in Lancashire is presented on the title page as the work of the Economic Research Section of the University of Manchester; the authors named in a prefatory note by Professor Jewkes are Mr. S. R. Dennison, Mr. W. L. Magraw, Mr. D. N. Chester and Mr. C. T. Saunders. It is in the main a study of unemployment and population statistics in the Lancashire Industrial Area as a whole and in its different regions since 1923. The area is broadly the whole of Lancashire south of Lancaster (excluding Merseyside) and the adjoining industrial fringes of Cheshire and Derbyshire. The first point emphasised is the "striking differences in the rates of unemployment in different parts of Lancashire." This is developed later in emphasis on two regions in particular—the mining district of the South West and the weaving district of Blackburn, Burnley and Rossendale—as presenting serious and intractable problems. Elsewhere variegation of industries has made readjustment possible under general trade recovery; in these two districts state assistance is still judged to be indispensable. The Wigan mining district is South Wales and Durham on a smaller scale. The weaving district is exceptional in concentrating in a single industry over one-third of all the insured males and four-fifths of the females. "It is impossible to avoid the conclusion that the weaving district, left to itself, is not likely to solve its problems by the growth of new trades. Its relative isolation will continue a drawback. But its chief difficulty lies in the absence of a large group of miscellaneous industries which would make it a good centre for other 'assembling industries'."

Nor is the problem likely to be solved by transfer of workpeople to other districts. Immobility is a dominating characteristic of labour which is so largely that of married women. Even on the optimistic assumptions that all wholly unemployed men aged 18 to 54, one half the wholly unemployed single and widowed women, and one third of the wholly unemployed married women of like ages are transferable, the proportion of the unemployed who could be transferred is only 50 per cent. in Lancashire, taken as a whole, and is only 42 per cent. in the weaving and spinning districts. In one town a live register of 2,500 women out of work yielded 120 whom it was worth interviewing with a view to offering them domestic work or training for it elsewhere and just four women who proved in the end both willing and able to accept the offer.

Immobility of labour perhaps is the most strikingly illustrated point in this book. But it is full of valuable information, clearly set out and well arranged.

The two volumes under review here carry on worthily the tradition of the Economic Research Section at Manchester in the scientific study of social phenomena and give hopes of many similar works to follow.

W. H. Beveridge.

Letters to Thomas Robert Malthus on Political Economy and Stagnation of Commerce. By Jean Baptiste Say. With an Historical Preface by Harold J. Laski. (Wheeler Economic and Historical Reprints, No. 2.) London: George Harding's Bookshop, Ltd. 1936. viii + 75 pp. 5s.

Say's well-known *Lettres a M. Malthus sur différent sujets d'Économie Politique, notamment sur les Causes de la Stagnation Générale du Commerce* appeared originally in Paris in 1820, and an English edition, "Translated for the Pamphleteer exclusively," was published in 1821 in vol. xvii, No. xxxiv, of that useful serial publication. The present reprint, except for the new introduction by Professor Laski and an abridgment of the title, follows that edition closely, including even the misprints, although it is not a photographic reproduction and the number of pages is increased from 57 to 75. The original pagination is not indicated. The translation also lacks the table of contents and the useful, although occasionally slightly comic (*e.g.*, "*Apparence*, trompe quelquefois, 5.") index of the French original. Like its predecessor in the series of Mr. Wheeler's reprints the volume is neatly produced and this time also reasonably priced. Since the translation is not very satisfactory it would perhaps have been even more useful if the original had been reprinted. But in view of the renewed interest in the subject of these letters and particularly in "Say's Law" expounded in them it is certainly welcome that this Classic of the literature of Economics should again be easily available in a convenient form. If readers will not all agree with Professor Laski's evaluation of the merits of the controversy or of the exact role played by the different participants in it, they should certainly be grateful for his valuable references to all the more important other works in which fuller information on it can be found.

F. A. HAYEK.

Prezzi e Mercati. (Collezione di opere scientifiche a cura della rivista "La Riforma Sociale.") By PASQUALE JANNACCONE. Guilio Einaudi, Turin. 1936. 375 pp. No price stated.

The purpose of this volume of reprinted articles is, Signor Jannaccone explains, to bring together his contributions both theoretical and statistical to the solution of the problems of imperfect competition. He considers that a thorough understanding of these problems is essential in a world becoming steadily more and more dominated by semi-monopolistic organisations. The first three articles in the collection, which were written before the War, are concerned directly with imperfect competition, but the last three only indirectly; considerations of space make it necessary to omit further reference to the fifth and sixth, which are of less intrinsic interest than the others.

In the first article, "Questioni controverse nella teoria del baratto," Signor Jannaccone surveys various discussions of the limits between which price is indeterminate under conditions of bi-lateral monopoly. He suggests that a suitable criterion for an arbritrated price would be that at which the monopolists' curves of estimated net gain from accepting an agreed price instead of fighting for a different one intersect.

It is unfortunate that Signor Jannaccone did not revise the second article, "Il dumping e la discriminazione dei prezzi," so as to eliminate a considerable section whose chief interest is dependent on the pre-war controversy on the relation between discrimination and the nature of the cost curves of which it formed a part. He might also with advantage have made good his neglect of the inter-relation between the prices and quantities sold in the different markets of a discriminating monopolist, if he had made use of the recent development of the marginal revenue analysis. Nevertheless the article is an important contribution to the literature on dumping. Signor Jannaccone's statement of the conditions under which discrimination is possible and his analysis of dumping simply as the particular case where markets happen to be divisible on geographical lines is admirable. So also is his classification of the types of dumping, distinguishing between dumping to get rid of temporary surpluses, to make possible a larger output and to force a rival to come to terms. He suggests that the second type may arise from the attempt of each firm in a competitive market trying to achieve equilibrium by expanding output until marginal and average costs equal price. This he argues, not altogether convincingly, leads to falling prices and hence to price agreements, and where possible dumping, rather than to the elimination of marginal firms.

In the succeeding article Signor Jannaccone deals with the advantages and disadvantages of dumping to different consumers, and the difficulty of protecting the home market against it. The most interesting feature of the article is, however, his demonstration that dumping naturally leads to the extension of price agreements. In the high-price market it forces industrial consumers to integrate vertically with the monopolist to protect themselves against the competition of producers in the low-price market, while in the low-price market competitors of the dumper are forced to come to terms and form horizontal price agreements.

The fourth article, "Relazione fra commercio internazionale, cambi esteri e circolazione monetaria in Italia nel quarantennio 1871–1913," is only indirectly connected with imperfect competition, but it is of considerable intrinsic interest. It is an excellent statistical study of the relations between the balance of trade, capital movements, the variation of the supply of money about the general trend which depends on the growth of population and industry and on the international price level, and the foreign exchanges. Signor Jannaccone shows that the apparent paradox of periods of small adverse balances and unfavourable exchanges alternating with periods of large adverse balances and favourable exchanges in Italy during this period merely indicated that Italy was dependent on foreign capital for her industrial development. The republication of this article in an easily accessible form must be welcomed by all students of international capital movements.

M. Bowley.

The Industrial Position of the North-East Coast of England. P. S. King & Son. 1935. 54 pp. 2s. 6d.

A Social Survey of Plymouth. P. S. King & Son. 1935. 36 pp. 1s. 6d.

Report on an Enquiry into Working-Class Family Budgets in Bombay City. Government Central Press. 1935. 44 pp. 4d.

The first of these three reports is prepared by the Economics staff of Armstrong College and supplements the 1931 Board of Trade Industrial Survey of the N.E. Coast. The industrial situation in the area is examined, over the period since 1929, in relation to that in the country as a whole. The Ministry of Labour supplied figures giving the numbers of insured and of unemployed workers in the various industries of the area and these, together with certain figures of industrial output, form the basis of the report. The results obtained serve to emphasise the conclusions of the 1931 survey. Unemployment on the N.E. Coast continues at a level twice as high as that of the country as a whole and the recent recovery has been much less marked. The six basic industries of the area still carry the bulk of the unemployment despite the fact that the proportion of insured workers in these industries has fallen off at an accelerated rate since 1929. The lighter industries, now accounting for nearly half the total of insured workers in the area, are in a much better position. But they supply needs which are mainly local and cannot be expected to show much more expansion. The N.E. Coast, it is clear, has not been successful in attracting new industries catering for wide markets.

The second report gives the preliminary results of a Social Survey commenced in Plymouth towards the end of 1933. The Survey was conducted by means of a sample enquiry, one house in every twenty in the Borough being selected for investigation, but it covers only working-class families. It should be noticed that the definition of a working-class family adopted (*i.e.*, a family with a total income of less than £5 per week) is different from definitions adopted elsewhere. For example, a family with two adult males in fairly skilled employment would be classed, on the Plymouth definition, as a non-working-class family. Some of the results are obviously affected by the adoption of this particular definition. The main topics discussed in the report relate to housing conditions and to family income. The Plymouth results can be compared with the results of other social surveys, *e.g.*, with those taken in East London, Liverpool and Southampton (1929–30). It appears that over-crowding is greater, and rents higher, in Plymouth than in the other areas (including East London) while family incomes are lower. For example, the surprisingly high average figure of 25 per cent. of income goes on rent in Plymouth as compared with only 14 per cent. in East London. On the other hand, the proportion of families below the " poverty line " is lower in Plymouth than in Liverpool or Southampton and about the same as in East London. This is accounted for by the greater number of what can be called standard families in Plymouth ; 48 per cent. of the families depend on a single adult male in regular work. We look forward to the full report to fill in the details of the picture of Plymouth's economic life provided in this preliminary review.

Between September, 1932, and June, 1933, the Bombay Labour Office collected about 1,500 family budgets in certain working-class districts of Bombay City and the results appear in the third of the reports reviewed here. The families investigated represent a 3 per cent. sample of those families with heads in regular work but otherwise taken as far as possible at random. A considerable amount of information on family income and expenditure was obtained. It is not clear whether the individual budgets relate to the whole of the September, 1932–June, 1933, period or only to some part of the period. In any case, it is to be regretted that the budgets do not cover a full year. The position of working-class families, as indicated by their modes of expenditure, shows some improvement on that of 1921–2, when the previous budget enquiry was made. The average proportionate expenditures on food and clothing have decreased while expenditures on miscellaneous and luxury items have increased. On the other hand, rent appears to account for a much higher proportion of income despite the fact that the bulk of the families still occupy but one room. These comparisons are only roughly valid since the 1921–2 enquiry included budgets from single men and from families with unemployed heads. It is significant that the proportionate food expenditure increases with income over the lower income ranges and only decreases at higher income levels. Food is still a " luxury " expenditure at low incomes. The miscellaneous expenditures show some interesting items. Almost all families record expenditure on hair-oil and on bangles and combs. Over 80 per cent. of the families spent on travel to their native place while not 30 per cent. record expenditure on other amusements. The report also provides much information on housing (which is incredibly poor on Western standards), on family composition, on occupations and on many other points.

R. G. D. ALLEN.

The Exchange Clearing System. By PAUL EINZIG. Macmillan, London. 1935. 220 pp. 8s. 6d.

However great the intellectual qualities of an author may be, even the most unbiassed of his readers would surely be inclined to subject to more than ordinary scrutiny the views of a writer in whose opinion " most of . . . the . . . adherents (of the orthodox school of economic thought) in their dogmatism would prefer to see perpetual poverty and even a collapse of civilisation on the basis of economic freedom rather than prosperity and progress at the cost of the curtailment of that freedom " (p. 15). Apart from ever recurring attacks on an initially false conception of *laissez faire* policy it is, after an analysis of the causes of the financial crisis, the author's purpose " to show that through the universal adoption of exchange clearing, mankind would be in a position to enjoy all the advantages of international trade and finance without having to endure their disadvantages " (p. viii). It cannot be said that the substance of the author's argument does much to dispel the impression created by its tone.

Two general points may be mentioned at the outset. The first concerns Dr. Einzig's interpretation of orthodox teaching in international trade theory. To prepare the way for his subsequent eulogy on the exchange clearing system, Dr. Einzig demonstrates the comparative unimportance of triangular trade in the process of the international settlement of accounts. Yet, not content with stating the legitimate orthodox objection to exchange clearing as a method of payment which tends to cut out triangular trade to the disadvantage of all trading nations, Dr. Einzig concludes that triangular trade has such a "strange fascination" for the *laissez faire* school that, "if it is true that (its) adherents would gladly sacrifice £10 millions of home trade for the sake of £1 million of foreign trade, it is equally true that they would cheerfully sacrifice £10 millions of bilateral foreign trade for the sake of £1 million of triangular or multilateral trade" (p. 172). The truth is, of course, that economists realise that £10 millions worth of home trade may in terms of commodities be worth no more than £1 million of foreign trade. This may sound exaggerated but would not Dr. Einzig's ratio be open to the same objection ? The second general point concerns Dr. Einzig's own views. Though it is conceded that "the system (of free trade) had unquestionably the advantage that under it goods are produced where they can be produced under the most favourable circumstances and at least cost," a few lines below we may read that "a higher degree of independence for a country means not only a higher degree of security against international crises but also an increase in the standard of living" (p. 48). What then is the ideal solution, one of maximum independence or one of extensive division of labour ?

The first part of the main argument (chapters VI to VIII on the immediate origin and technical structure of clearing agreements) shows considerable study of the provisions of existing agreements, and it is definitely to the credit of the author that in this connection he draws the attention of the antagonists of exchange clearing to the fact that before its introduction the choice lay, in many cases, between trade *via* clearings or no trade at all. In the second part (chapters IX to XIII on the effects and technical problems of exchange clearing), the reader looks in vain for a more thorough analysis of the economic problems raised by clearing trade, such as the reasons for those changes in net balances between the contracting countries which have in so many cases seriously endangered the functioning of the agreements.

The exceedingly reasonable suggestion of multilateral exchange clearing under *existing* conditions, contained in the third part (chapters XIV to XXV chiefly on multilateral exchange clearing), is spoilt in two ways. First, in view of the history of exchange clearing during the past few years, there is, contrary to Dr. Einzig's view, no reason to believe that the existence of bilateral trade accounts (justly retained as a basis for a workable system of multilateral clearing) makes trade necessarily more stable than free multilateral settlement of international accounts. Secondly, the analogy between multilateral exchange clearing and internal bank clearing must be used with more caution than has been shown by Dr. Einzig. International clearing does not involve funds of the same nature as is the case with internal clearing, where a pound is a pound and pengo a pengo, but currencies of different degrees of transferability although the official values may not have changed. Thus the advantages to be derived from international clearing are, even under existing conditions, severely limited by the very conditions which brought about the introduction of the clearing system : the non-transferability of many of the world's currencies.

Yet, while it is one thing to advocate exchange clearing once a large number of countries have surrounded themselves with a wall of exchange restrictions or to suggest ways and means for the multilateral clearing of existing bilateral balances, it is quite another to advocate the introduction of such a system as a permanent ideal solution of international financial problems. Dr. Einzig's implicit claim seems to be that the exchange clearing system would combine the advantages of both the gold standard and a system of free paper currencies. The limits which are set by the operation of the gold standard to a country's policy of expansion are considered undesirable (*passim* and p. 13). The exchange clearing system would be the ideal solution since under it " there would be no need to contract credit if a country lost gold or if its clearing accounts showed a persistent debit balance . . . Such disequilibrium as might arise from time to time could be corrected if necessary by a corresponding adjustment of the rate of exchange " (p. 219). On the other hand " exchange clearing and the gold standard are by no means incompatible," (p. 215), and gold reserves would " be valuable . . . as tending to preserve confidence in the note circulation and the credit structure." But Dr. Einzig cannot have it both ways. Either gold is to serve as a means towards confidence at home and abroad, in which case the note circulation and the credit structure must in some way be determined by the gold stock and the assurance must exist that exchange rates are not liable to fluctuation, or else the international balancing of accounts must take place through movable exchange rates, in which case, if Dr. Einzig's suggested possibility to " cast aside . . . excessive timidity in matters of finance " (p. 219) were taken advantage of, it would be difficult to agree with him that the exchange clearing system would bring about that degree of stability in international economic affairs which supposedly justifies its universal introduction.

In conclusion, let it be added that, in spite of the occasional looseness of language apparent in the League of Nations Enquiry on Clearings, even the most scathing attacks which Dr. Einzig has found appropriate to hurl against this report do not seem to invalidate the chief findings of its authors.

H. K. HEUSER.

Der Tee im Britischen Weltreich. By FRITZ KLOPSTOCK. Hermann-Büxenstein, Berlin. 1936. 85 pp. No price given.

In this doctorate thesis Mr. Klopstock traces the origin and development of the tea industry and trade, describes the different kinds of tea and their preparation for the market, discusses the predominant part played by the British Empire in world production and consumption, describes the London tea market, discusses the relation between demand, supply and price, and analyses in detail the market position during the last fifty years with special reference to the course of the trade cycle, tariffs and restrictions. The statistics are well chosen. The reader is warned against the inaccuracy of war-time statistics, especially in Great Britain!

Some of the most interesting conclusions refer to the elasticity of the demand for tea, the effects of Imperial Preference, the relative effects on retail prices of changes in wholesale prices and in the British tariff on tea, and the working and results of restriction. Although the conclusions themselves are in no way startling, the author has rendered a signal service by setting forth clearly and concisely the statistical and other evidence on which they are based. For instance he shows that a rise in price of the order actually experienced in recent years leads not so much to economies in the consumption of tea, or a substitution of some other beverage for tea, as to the purchase of cheaper types, which itself leads producers to mix better quality teas with poorer types, and hence to a lessening of the price difference between different qualities.

Imperial Preference has led not to any substantial expansion of the total market for Empire teas, or the realisation of larger profits, but to the diversion of Empire teas from foreign and colonial areas which grant no preference (such as Australia) to the British and other Imperial markets, where preference is given, and to the undercutting of Empire by foreign (especially Java) tea in the former markets. The author concludes that, so long as the Irish Free State and Canada and New Zealand (as well, of course, as Great Britain) grant Imperial Preference, a slight advantage accrues on balance to Indian and Ceylon teas.

Figures of changes in wholesale and retail prices show that short-term changes in auction prices, and even long-term, but gradual, changes in such prices, leave retail prices practically untouched, whereas sudden changes in the British tariff are quickly reflected in retail prices.

Restriction has been carried out under especially favourable circumstances in the case of tea. Large scale capitalistic production and control prevail: native producers play a subordinate part, except in Java: there is substantial coincidence of interests between producing and consuming areas, as over seventy per cent. of output and consumption are concentrated within the British and Dutch Empires; the relatively quick loss of flavour and hence of value by tea tends to prevent the withholding of stocks from sale; whilst the output of countries (*i.e.*, Japan and China) not participating in restriction, amounts only to about twenty per cent. of total production, and is also clearly differentiated in flavour from the output of restricting areas, so that competition therefrom is reduced to a minimum. In addition the objects and methods of restriction have, at least in the scheme of 1933, been moderate and efficient. The result is that the scheme has worked smoothly, without engendering rapid changes or substantial fluctuations in prices. The conclusion is reached that conditions of supply and demand render it improbable that restriction can be withdrawn in the near future without serious effects on the profits of the industry.

Rather more might, perhaps, have been said about the problem of labour supply in India and Ceylon, and about the advertising schemes that have been tried, so far with small success, in various countries including the United States. The bibliography might with advantage have been extended to include more references to the work of agricultural economists, such as Dr. Harold Mann, which are to be found in such publications as the Journal of the Royal Society of Arts, the Journal, Transactions and Proceedings of the Agricultural Horticultural Society of India, and in the official publications of the Government of India. Further references might also have been given to official publications on the immigration of labour into Assam. A few minor errors may be noted. For instance on page 48, "eine Spur" should read "keine Spur"; on page 84 the population of India is erroneously given as 320 millions; and in the bibliography Sir William Beveridge's initials are transposed. On the whole, however, the book can be thoroughly recommended to all who want a sober and scientific account of the whole subject.

VERA ANSTEY.

Lectures on Political Economy. By KNUT WICKSELL. Volume II. *Money.* Edited by LIONEL ROBBINS. London. George Routledge & Sons, Ltd. 1935. vi + 238 pp. 8s. 6d.

This volume on Money should be of great use to students, as it contains a scholarly and lucid exposition of many problems of monetary theory. These lectures are indispensable to anyone who is at all interested in the development of monetary theory. The importance of the contribution which Wicksell made in this field of economic science needs no emphasis. Wicksell had such an acute mind that his writings are still a great stimulus to thought, although his work on money has been the subject of so much discussion.

The value of money, and money and economic equilibrium, are the two main problems with which monetary theory deals. Wicksell's discussion of both these questions is important. One cannot agree with his statement on page 20 that, " . . . . ' supply ' and ' demand,' expressions so conveniently applied to almost everything under the sun, become obscure and, in reality, meaningless when applied to money," but, nevertheless, his analysis of the supply conditions of money is very good. Wicksell's definition of money excludes credit instruments. Credit is considered by him to have increased the velocity of circulation of money. An increase of credit is said to raise the " virtual velocity of circulation " of money. It is no doubt more convenient, given present institutional arrangements, to consider the supply of credit as increasing the total supply of money rather than increasing the velocity of circulation of currency. The choice of definition does not, however, affect the logic of the analysis. Wicksell's theory of bank deposits is modern. He realised that banks could create deposits, but he also understood the limitations imposed on the credit-creating powers of an individual bank or a banking system. His discussion of the supply conditions of money includes his famous theory of the money and natural rates of interest. In this connection, there is an excellent summary and criticism of the Currency and Banking Schools.

It is now well known that Wicksell's identification of a stable value of money with monetary equilibrium is not justifiable, but the debt which modern economists owe to Wicksell's analysis of the money and natural rates of interest cannot be exaggerated. Wicksell, himself, refused to recognise his monetary theory as a theory of the trade cycle. He writes on page 211, " The principal and sufficient cause of cyclical fluctuations should rather be sought in the fact that in its very nature technical and commercial advance cannot maintain the same even progress as does, in our days, the increase in needs—especially owing to the organic phenomenon of increase of population—but is sometimes precipitate, sometimes delayed." The fundamental idea of modern Austrian trade cycle theory is, nevertheless, to be found in Wicksell. On page 207 he writes of " good times " when everyone believes he can earn a good profit. He points out that the real cause of the prospects of gain is " doubtless the gain already obtained in certain enterprises, as a result, for example, of technical or commercial progress." The following sentences are worth quoting verbatim. " The real rate of interest, therefore, is high, and is expected to remain so in the immediate future, whilst the loan rate remains for the moment unchanged. The element of a rise in prices is therefore present, according to our theory, but it is equally clear that sooner or later the banks will be induced to raise their rates, since the technical discoveries have not brought them any additional supplies of money and neither the velocity of circulation of money nor the perfection of banking technique can be raised to an unlimited extent. Higher prices and an increased volume of business, on the contrary, require a larger amount of hard cash or bank notes in circulation."

J. C. Gilbert.

An Introduction to Economic Analysis and Policy. By J. E. MEADE. Oxford University Press. 392 pp. 10s. net.

"I have often found," writes Mr. Meade, "that men, who come to economics to find an answer to the pressing problems of the moment, cannot see that the formal analysis of the text-book has anything to do with the real world." He therefore combines his analysis with a discussion of economic policy, continually pausing in his climb up the mountain of economic theory to show us the route to the promised land. But the outlines of this land, which Mr. Meade thinks so desirable, remain somewhat vague. One gathers that he would like to take us into the realm where the State owns all property and all incomes are equal. If we cannot get so far he would at least move us into an economy in which full employment is provided through monetary policy and public works, monopoly is controlled and curbed by the State, and inequality of income is diminished through the machinery of Public Finance.

Even those who sympathise with Mr. Meade's social ideals, and are in favour of presenting analysis as the instrument of a particular policy, may not agree with all his proposals. For example, they may not agree that all labour incomes should be exempted from income-tax, and they may not like his readiness to permit a series of budget deficits.

The present reviewer was somewhat alarmed by the great stress laid on unemployment. The first condition to be fulfilled before any economic system can provide the highest possible standard of living is that "no economic resources, which are willing to work, must stand idle." "The most glaring economic absurdity at present is the existence of unemployed resources." But surely Mr. Meade would not contend that all land should be cultivated, that all mineral deposits should be worked, and that equipment in good technical condition should never be scrapped or left idle? The principle here is that certain factors are best left idle if the other factors required to co-operate with them can produce a greater value in other uses. If this principle is admitted, it applies equally to workers. Given further complications, such as obstacles to mobility, it is probable that there is nearly full employment—in the sense of Mr. Keynes—in this country at present. This means that the loss to production caused by unemployment is serious only in times of great depression, and even then amounts to quite a small percentage. Yet Mr. Meade makes unemployment the theme of Part I of his book and considers that the chief aim of economic policy should be to employ all resources—even, it would seem, if this involves sacrificing other desirable aims.

The book as a whole is very clearly written. It will be particularly helpful to those who want a good summary of recent developments in the theory of imperfect competition and it may be useful as an introduction to the views of Mr. Keynes on the theory of money and employment. But to call it an introduction to economic analysis is simply misleading the consumer.

FREDERIC BENHAM.

The Improvement of Commercial Relations between Nations. The Problem of Monetary Stabilisation. International Chamber of Commerce, Paris. 1936. 417 pp. 16s.

International Economic Reconstruction. International Chamber of Commerce, Paris. 1936. 225 pp. 8s.

These two volumes constitute a survey made by a committee of experts under the auspices of the Carnegie Endowment for International Peace and the International Chamber of Commerce. The first volume contains memoranda by various economists, most of them well-known, on particular aspects of these two problems. The second volume contains a report by Professor Ohlin on international economic reconstruction and a report by Professor Gregory on monetary stabilisation. Both volumes are well worth reading. Some of the memoranda in the first volume are particularly valuable and enlightening.

The committee recommend an agreement on monetary policy such as was in fact concluded between Great Britain, the United States, and France, soon after these reports were published. But they seem averse from a general return to a *de jure* gold standard. In so far as their aversion springs from the existence of refugee capital, which may move between centres and disturb exchange rates, it seems mistaken. The potential amount of refugee capital is enormous, for the nationals of a country may wish to transfer their assets into another currency. Surely the best way to cope with this problem is to remove the motives for movements of refugee capital by a general return to the gold standard.

FREDERICK BENHAM.

Patterns of Economic Activity. By ARTHUR RADFORD. Routledge. 12s. 6d.

There is little that is new in the ideas expounded in Mr. Radford's book. Their presentation, on the other hand, is novel, interesting and useful. Its thesis, from the pedagogical point of view, is that general equilibrium and the concrete facts of the economic organisation (however simplified for expository purposes) should be studied before particular equilibrium. This conclusion is based on some years of teaching experience, as is Mr. Radford's peculiar technique of graphical exposition.

Economic activity is described as a process of pattern making. " Every activity of which man is capable involves the use of physical or material resources " even though these may be no more than his own body and the spot of ground which it occupies ; and " the whole process of what we call ' living ' may be regarded as a process of making patterns of particular things." Economics is " the study of the whole of human living from one point of view, that from which man is seen making patterns of material resources." The essence of pattern-making is administration, that is choice between alternatives or substitution. Mr. Radford does not accept the criterion of scarcity to differentiate between economic and non-economic problems. His defence of this point of view is interesting but not convincing. Plethora, he says, is as important as scarcity; all economic activity consists in adjusting relative scarcity and relative plethora; why give priority to scarcity? No one would deny that relative plethora is as "important" as scarcity, all economic quantities being relative; but after all plethora of one thing merely means scarcity of that thing's opposite. A plethora of food implies a scarcity of appetite; a plethora of appetite, if one cares to use the expression, is certainly one of the major springs of economic activity, but the advantage of this terminology is hard to see. The "administration of resources" definition of economics, jettisoning the scarcity criterion, falls back into the confusion between technical and economic problems from which the scarcity concept has rescued methodology.

Mr. Radford himself, however, is not misled, and avoids stumbling into the morass of hyphen-*wirtschafts* which he has reopened to the unwary. His opening chapters, in which he uses the notion of pattern-making to clarify the nature of property rights, the distribution of work and leisure, and the constitution of economic well-being, are admirable. Patterns serve him well in the discussion of capital theory (introduced surprisingly early in the book) and in that of the flow of exchange value; the diagram on p. 97, with its accompanying text, should have a curative effect in the most acute cases of Social Credit. This chapter, indeed, provides a useful framework for more advanced studies of purchasing power problems—a study which lies outside Mr. Radford's present intention.

Valuable as are many of Mr. Radford's expository devices, and little as his initial methodological shortcomings matter to him in practice, his book is disappointing as a whole. He writes lengthily and flatly, and his chapter on money and banking is frankly confusing. The passage from general to particular equilibrium is clumsy, and the avowed object of linking up modern and classical theory imperfectly attained. Altogether, *Patterns of Economic Activity* is far better as an aid to teachers than as a foundation textbook for students.

HONOR CROOME.

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The Changing Structure of the Family in Australia

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I

Introduction

The existence of declining fertility in all highly industrialised countries is now well known. Since fertility in Western and Northern Europe and the United States has reached a level which, if maintained, must eventually lead to a declining population, it is now advisable to analyse in greater detail the changes which have taken place in the reproductive behaviour of people in these countries. Before an effective programme can be devised to arrest or reverse falling fertility, we need to know how far the fall has been accompanied by a reduction in the number of large families and to what extent it entails an increase in the number of women having no children. The present paper is mainly an attempt to throw some light on these two questions, and, in so doing, to ascertain the proportions of families of various sizes appropriate to different fertility levels. Such considerations are fundamental to a housing programme designed to arrest population decline.

Owing to the defective system of birth registration it is impossible to get direct information concerning Great Britain. The country selected for study is the Commonwealth of Australia, over the period 1909 to 1933. For any study of size of family from year to year the essential data are records of births by age of mother and order of birth of child. For the Commonwealth of Australia the data are first given for 1909, so that a period of twenty-five years has been covered. Details of the data and methods employed will be found in later sections.

At the beginning of the twenty-five year period fertility was rising, though doubtless it had already fallen below the level of an earlier period of Australian history. During the period studied the highest point was reached in 1912, with a gross reproduction rate of 1.781. From this year fertility fell steadily except for the post-war recovery, till in 1933 the gross reproduction rate was 1.052, a drop of 41 per cent. in twenty-one years, so that in the period covered fertility changed from a figure characteristic of a quite rapidly increasing population to one just too low to maintain a stationary population. In 1933 the net reproduction rate was 0.96. In 1931 it was 1.03, i.e. in that year fertility and mortality were in equilibrium.

During the twenty-five year period, the population of Australia increased from 4,300,000 to 6,600,000.[1] In the early years there was a large inward balance of migration, reaching its maximum in 1912, when the rate of increase by migration is given as 2.04 per cent.[2] of the total population as against a natural increase of 1.7 per cent. During the war there was outward migration which ceased in 1919. Since then inward migration maintained a fairly steady level until 1929 of about 0.65 per cent. of the total population. Since 1930 there has been a slight excess of departures over arrivals.

There are many aspects of Australian history which might throw light on the problem studied. Only two will be mentioned. There has been throughout a steady increase in the proportion of the population living in cities. The metropolitan population is given as 38.53 per cent.[3] in 1909 and 46.87 per cent. in 1933. Employment of women fluctuated considerably during the period. It was at a maximum in the year 1922–23 when 386 women per 10,000 were returned as employed in factories. The proportion was nearly as large in 1933, when it was 379. It was at a minimum in 1930, when it was 291.[4]

Needless to say, the greatest interruption to the normal course of events was provided by the World War. Among other effects, 329,883 men served overseas and the number of war deaths was 59,342.[5] The changes in fertility will be examined in detail later. Throughout this communication fertility and sterility are used in a purely statistical sense, as referring to women who have or have not borne children.

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[1] *Australian Demography. Bulletin No.* 51 (1933).

[2] *Ibid.*

[3] *Ibid.*

[4] *Official Year Book.*

[5] Data obtained from Australia House.

II

The Change in Fertility

The method of calculating the gross reproduction rate has been fully set out by Kuczynski.[1] For Australia the yearly births by age of mother in five-year age periods were obtained from the Demographic Bulletins. The age composition of the population was only obtainable for the census years 1911, 1921 and 1933. For the intervening years it was obtained by graphical interpolation and is therefore subject to error. The maximum possible error due to this cause in the gross reproduction rate for the intercensal years around 1916 and 1927 would be about 3 per cent. The occurrence of censal years at the beginning, middle and end of the period makes this possible error of little significance in studying the trend of fertility. The ratio of women in the reproductive period to total population and also the ratio of younger to older women were highest at the beginning of the period. The proportion of women between 15 and 50 fell in 1921, but later recovered, so that in 1933 it was 53.6 per cent. of the total female population, nearly as high as in 1911. The ratio of younger to older women, however, continued to diminish. In 1911 there were 2.1 times as many women 15 to 20 as women 45 to 50, but in 1933 only 1.5 times as many. The 1921 Census showed the largest number of women in the age group 25 to 30, whereas the other two censal years showed the largest number in the age group 15 to 20.

Table I sets out the gross reproduction rate for each year from 1909 to 1933. The change in the total gross reproduction rate is shown graphically in Figure I.

The ex-nuptial rate is chiefly of interest as showing the proportionately greater falling off in illegitimate births. The illegitimate births in later years appear to have been replaced to some extent by births conceived before marriage. In Australian statistics the terms " ex-nuptial " and " nuptial " are used in place of " illegitimate " and " legitimate ". The latter terms will be used in the text as being more familiar to readers in Great Britain.

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[1] *Fertility and Reproduction.*

TABLE I

GROSS REPRODUCTION RATE. AUSTRALIA, 1909—1933

Year	Ex-Nuptial	Nuptial	Total
1909	.087	1.575	1.662
1910	.085	1.569	1.654
1911	.087	1.606	1.692
1912	.087	1.694	1.781
1913	.085	1.660	1.745
1914	.081	1.651	1.732
1915	.078	1.572	1.651
1916	.069	1.512	1.580
1917	.070	1.469	1.539
1918	.074	1.401	1.475
1919	.070	1.341	1.410
1920	.069	1.466	1.535
1921	.069	1.438	1.507
1922	.065	1.430	1.495
1923	.066	1.379	1.445
1924	.064	1.363	1.427
1925	.063	1.342	1.405
1926	.062	1.287	1.349
1927	.059	1.269	1.328
1928	.059	1.250	1.310
1929	.056	1.196	1.252
1930	.054	1.180	1.235
1931	.053	1.074	1.128
1932	.047	1.007	1.054
1933	.047	1.005	1.052
	PERCENTAGE	FALL	
1912—1933 ..	46%	41%	41%

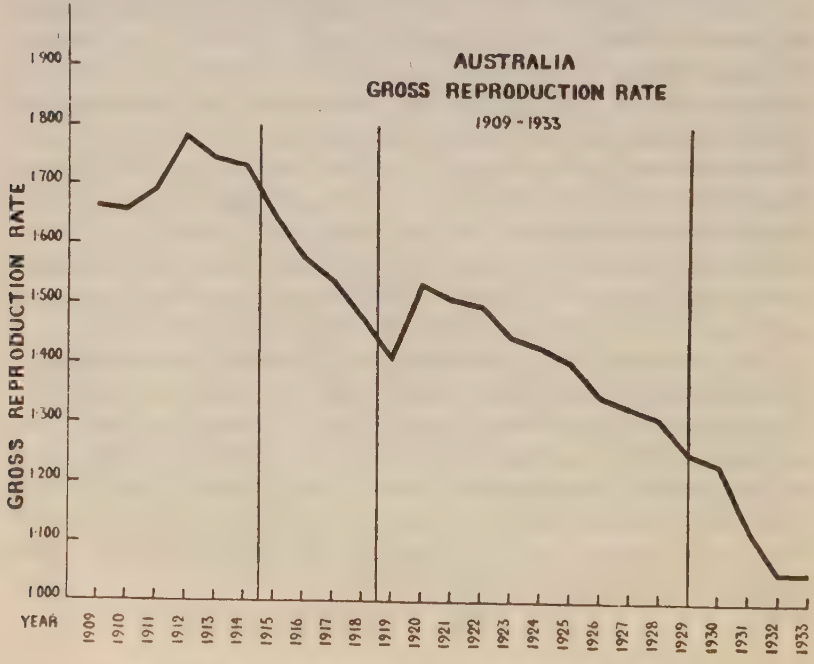

FIG. 1. The change in the gross reproduction rate, 1909 to 1933.

From 1909 onwards the pre-war and war-time fall in fertility was associated with little change in the mean age of mothers at birth of all children. This was because, while fertility of the older age groups fell rapidly, there was also a considerable fall in the 15 to 25 age group together with a considerable reduction in the illegitimate birth rate which mainly affects this age group. Since 1921, the mean age of mothers has fallen from 29.3 years to 28.3 years. This is because fertility has not fallen greatly in the younger age groups. In the 15 to 20 group, fertility in 1933 was practically the same as in 1920 and during the intervening period had been much higher. Table II shows the percentage fall in specific fertility rates.

TABLE II

PERCENTAGE FALL IN SPECIFIC FERTILITY RATES 1912—1933

Age Group	Ex-Nuptial	Nuptial	Total	Percentage Ex-Nuptial fertility of Total in 1933
15—19 ..	40.3	2.6	16.2	20.9
20—24 ..	50.6	30.8	32.6	6.4
25—29 ..	55.7	38.8	39.3	2.5
30—34 ..	42.0	45.2	45.1	2.2
35—39 ..	34.6	52.9	52.7	2.5
40—44 ..	35.9	57.7	57.3	3.0
45—49 ..	43.0	62.4	61.9	4.4

It will be seen that the legitimate fertility of the 15 to 19 age group has fallen very little. The fall in total fertility is almost wholly due to the fall in illegitimate fertility. Illegitimate births are most important in this age group, since in 1933 30 per cent. of all illegitimate births were due to mothers under 20. The number of illegitimate births to women over 35 is so small that the differences in the percentage fall in illegitimate fertility in these age groups are probably not significant.

III

CHANGES IN THE SIZE OF THE FAMILY

(*a*) DATA AND METHODS

The data available consist in the main of an annual table of order of birth in the Demographic Bulletins entitled " Birth Total, Mothers' Ages, Duration of Marriages and

Issue". The table includes married mothers only; ex-nuptial children only affect the order of birth of later children and do not themselves appear in the table. The principal omission is therefore that of ex-nuptial children whose mothers afterwards marry and have legitimate children by the same father and who may therefore be regarded for all practical purposes as part of the legitimate family. Further it should be noted that the table records current marriages only and takes no account of former marriages. The method for dealing with these figures was suggested by Kuczynski. It involves first calculating gross reproduction rates in the usual way for first, second, third, etc., children respectively. For each thousand women between 15 and 50 we thus obtain the number who will have in the course of their lives a first child, a second child, etc., on the basis of the fertility of a given year. By subtraction the size of the families of a thousand women can be obtained. For example, if it is found that out of every thousand women 800 will have a first child, 700 a second and 500 a third, it is clear that out of every thousand women there will be 200 with no children, 100 with a family consisting of one child only, and 200 with a family of two children only, while 500 will have families of three or more children. While the gross reproduction rates given in § 2 were calculated for female births only as is customary, the rates given in this and later sections are based on all births since the main object is to ascertain family sizes.

Tables III and IV show the results thus obtained from the tables cited. They refer only to children legitimately born of a single marriage. They take no account of illegitimate children except in so far as they affect the order of later-born children. Modifications will be dealt with later, but as these involve estimates which can at best be only very rough approximations the data are presented first in their simplest form which directly depend on accredited data. Owing to great fluctuation in the rates for first and second children, as will be described later, the rates are presented as five-yearly averages round the most suitable years.

(*b*) ILLEGITIMACY AND REMARRIAGE

The size of family so obtained does not represent any very clear cut sociological entity. Some attempt must be

TABLE III

REPRODUCTION RATES PER 1,000 WOMEN BY ORDER OF BIRTH. FIVE-YEARLY AVERAGES

(Nuptial children of single marriages only)

Order of birth of child	1909—1913	1911—1915	1922—1926	1925—1929	1929—1933
1 ..	793	835	769	744	660
2 ..	626	654	618	578	528
3 ..	502	508	434	409	351
4 ..	388	385	296	281	235
5 ..	291	284	207	189	157
6 ..	214	209	146	128	106
7 ..	157	151	103	91	71
8 ..	109	104	71	62	48
9 ..	76	70	48	41	31
10 ..	51	47	31	27	19
11 ..	31	27	17	16	12
12 ..	18	16	10	8	7
13 ..	10	9	5	4	4
14 ..	5	4	2	2	2
15 and over ..	4	3	2	2	1
Total G.R.R.	1.707	1.720	1.424	1.329	1.144

TABLE IV

SIZE OF FAMILY PER 1,000 WOMEN, FIVE-YEARLY AVERAGES

(Nuptial children of single marriages only)

Size of family	1909—1913	1911—1915	1922—1926	1925—1929	1929—1933
0 ..	207	165	231	256	340
1 ..	167	181	151	166	132
2 ..	124	146	184	169	177
3 ..	114	123	138	128	116
4 ..	97	101	89	92	78
5 ..	77	75	61	61	51
6 ..	57	58	43	37	35
7 ..	48	47	32	29	23
8 ..	33	34	23	21	17
9 ..	25	23	17	14	12
10 ..	20	20	14	11	7
11 ..	13	11	7	8	5
12 ..	8	7	5	4	3
13 ..	5	5	3	2	2
14 ..	1	1	–	–	1
15 and over ..	4	3	2	2	1

made to estimate the number of illegitimate children whose parents later marry. The only data which bear directly on this point are the number of legitimations under the

Legitimation Acts of the various states. These were first recorded in the Official Year Book for 1929 for the years 1924 onwards. It is of course impossible to say in what years the births occurred which were legitimatised in any particular year. Relating the legitimations to births of the same or the previous year they range from 15.6 per cent. to 14 per cent. of all illegitimate births, but relating them to the births of two years earlier, the range is lowered to 13.6 per cent. The best approximate figure for the number of illegitimate births afterwards legitimatised seems to be 14 per cent. This figure has been applied throughout the period, though possibly the proportion of children who later became members of a legitimate family may have been greater at the beginning of the period. The question of remarriage will be discussed later, and it will be shown that it does not greatly affect the size of the family. For the present the addition of a percentage of illegitimate births gives a table of families which represent definite biological and sociological entities, i.e. the families produced by two parents who at some time or another marry. Apart from remarriage the families of women who do not marry are also omitted. The illegitimate birth rate in Australia is not very large. In 1909 it was 6.01 per cent. of all births and fell fairly continuously to 4.71 per cent. in 1933. The remaining 86 per cent. of illegitimate births will be considered later, but for the present they are omitted from the discussion of size of family. Table V sets out the figures obtained by the addition of legitimatised children to the figures in Tables III and IV.

TABLE V

REPRODUCTION RATES PER 1,000 WOMEN AND SIZE OF FAMILY CORRECTED FOR LEGITIMATISED CHILDREN

Year	1909—1913	1911—1915	1922—1926	1925—1929	1929—1931
First children..	817	859	787	761	675
No legitimate children ..	183	141	213	239	325
One child ..	191	205	169	183	147

A possible source of error is that more than one illegitimate child of the same parents may later form part of a legitimate family. No account has been taken of this, but there is some reason to think that the error involved cannot be large. In Czechoslovakia in 1925 the proportion of children legitimatised who were first-born appears to have been 89 per cent. as compared with 67 per cent. first births among all illegitimate births. We should therefore expect the percentage of first births among legitimatised children in Australia to be about 90 per cent. at the beginning of the period and considerably higher at the end (vide § 5 (*b*)). Considering the number of mothers of illegitimate first births not later legitimatised as shown in § 5 (b) we can apply the current ex-nuptial infant mortality death rate to these births. Assuming then that the mothers of illegitimate children who die under a year old are as likely as other women to marry and have legitimate children, it appears that this would reduce the number of legitimate first births at the beginning of the period by about 1 per cent. and by less than 1 per cent. at the end of the period. The maximum error due to this source thus appears to be 1 per cent. in the pre-war period only. Against this could be set the fact that legitimations may have been more than 14 per cent. in this period. In the pre-war period there was no legal legitimation; but it has been assumed that illegitimate children whose parents later married occupied a family status which was nearly equivalent to that enjoyed by those who at a later period were legally legitimatised.

The tables presented indicate a significant trend over the period with several features which are sufficiently clearly marked to be outside the limits of any statistical error. It will be seen that the proportion of families of only children has not increased but on the other hand has become slightly less. Looking at this from another angle, the ratio of second to first births for the 1911 five-year period was 77 and for the 1931 five-year period was 78. Both these rates fluctuate considerably from year to year; but it seems safe to say that there appears no tendency for the proportion of women who having had one child, follow it up with a second, to diminish. After the second child the rates rapidly diminish, giving an increase in families of two children only and a marked diminution in the numbers of large families. The difference between the proportions of families of various types in 1911 and 1931 is shown in the form of pyramids in Figure 2. Perhaps the most significant feature of the change is the emergence of the fashionable family of two children.

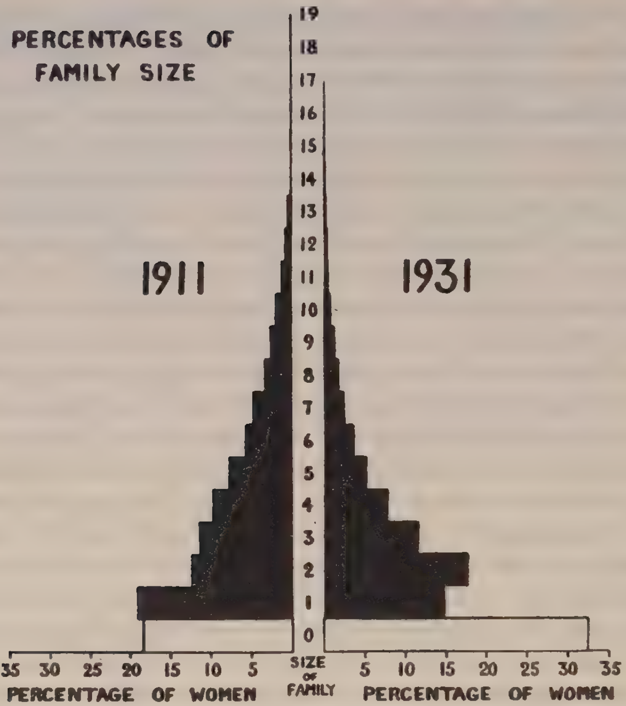

FIG. 2. Percentages of women having families of different sizes at current fertility rates, 1911 and 1931 (five-yearly averages).

The data can be presented in another form by considering only women who have at least one legitimate child. Table VI sets out the percentages of such women having families of different sizes in the two five-year periods 1909–1913 and 1929–1933.

TABLE VI

PERCENTAGES OF DIFFERENT SIZES OF FAMILY AMONG 100 WOMEN HAVING AT LEAST ONE LEGITIMATE CHILD

Size of Family	1909—1913	1929—1933
1	23	22
2	15	26
3	14	17
4	12	12
5	9	8
6	7	5
7	6	3
8 or more ..	13	7

Table VI illustrates the same points that were brought out in previous tables. There is no significant change in the proportion of one child families, a great increase in the proportion of two child and to a lesser extent of three child families, while the number of families of seven or more has been halved.

(*c*) Completed Marriages

The relation of fertility to population growth is most readily grasped when the former is referred to the size of completed families. If the gross reproduction rates were stable for sufficiently long they would, of course, have a simple relation to the number of children in all completed families. The fact that they are based on figures which change from year to year gives them a somewhat artificial nature. It therefore seemed desirable to make some attempt to compare the trend of the gross reproduction rates calculated for the period with the results of completed marriages. Previous studies of completed marriages have been open to the objection that the survivors to an advanced age of a group of marriages may be a highly selected sample. The present study does not appear to be open to that objection though complicated by other factors, particularly those due to high migration. Nevertheless it is hoped that it may prove suggestive for purposes of comparison. The annual Demographic Bulletins give tables of the issue of deceased parents. These cannot be used to give completed[1] families since for any one year and for a given duration of marriage they represent a selected sample of the marriages contracted.

The annual births in Australia are recorded for duration of marriage as well as for order of birth and age of mother. It is therefore theoretically possible by working backwards to tabulate, by order of birth, all the births resulting from the marriages of any particular year. This can only be done for three years, 1909, 1910, 1911, marriages in these years being at least 99 per cent. complete by 1933. The ratio of second to first children can, however, be traced for some years later. During these years there was a considerable inward balance of migration of married women, some with children, so that some of the marriages and some of the first children corresponding to the births of second children will not have been recorded in Australia. Table VII sets out the results obtained by this method.

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[1] As the word is used here the family is completed if further issue is stopped either by the natural cessation of fertility or by the termination of the marriage by divorce or death. The families obtained refer to current marriages only and no account is taken of remarriage.

TABLE VII

PERCENTAGES OF DIFFERENT SIZES OF FAMILIES RESULTING FROM MARRIAGES OF 1909, 1910, AND 1911

Size of Family	1909	1910	1911
0	12.9	12.4	10.5
1	10.7	11.4	15.5
2	16.2	17.1	17.6
3	17.2	17.7	17.7
4	13.4	13.7	13.3
5	10.1	9.6	8.9
6	6.7	6.4	6.0
7	4.5	4.3	4.1
8	3.1	3.0	2.6
9	2.2	1.9	1.7
10	1.2	1.2	1.1
11	.8	.6	.5
12	.5	.4	.2
13	.2	.2	.2
14 and over ..	.3	.2	.1

Gross reproduction rates do not distinguish between childless women who are and are not married. Hence even when appropriate correction is made for illegitimacy the figures obtained from them are not comparable with the ratio of childless and one child completed families. Comparison can only be made between the ratios of second to first children and so on. In Fig. 3 the families resulting from the marriages of 1911 are compared with those derived from the reproduction rates—(*a*) the five-yearly average for 1911 and (*b*) the 1933 rate. As all the women married do not live to the end of the reproductive period it would be more correct to make the comparison with net reproduction rates. The figure therefore shows also the 1911 net reproduction rates (males and females). But while the substitution of net for gross reproduction rates materially affects the proportion of first children born, as will be seen, it does not greatly alter the ratios of second to first children, etc.

Comparing the completed families of marriages contracted in 1911 with reproduction rates according to order of birth in 1911 and 1933, we find that the ratio of second to first children was higher than that shown by the reproduction rates in either year. This may have been due to the high immigration at this period. The ratio of third to second children was very near to the 1911 rate. Later ratios diverged from the 1911 rates until sixth to fifth and later ratios were very close to the 1933 rates. The

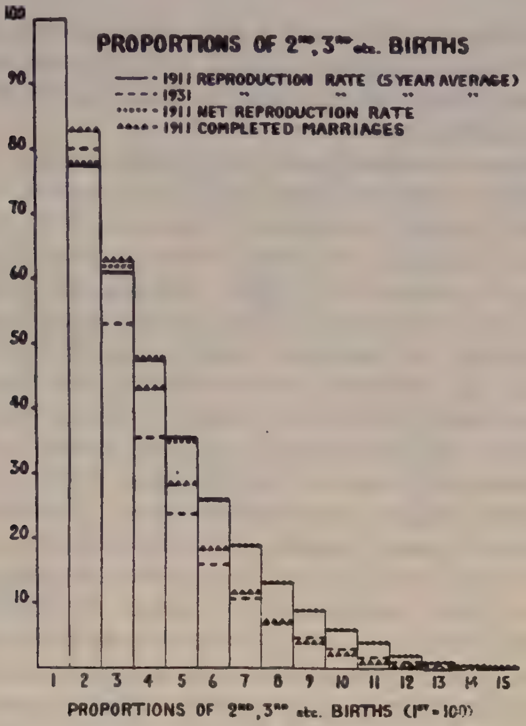

FIG. 3. Proportions of 2nd, 3rd, births to 1st in completed marriages of 1911 and according to 1911 and 1931 reproduction rates.

fact that the later ratios are lower than would be expected may be accounted for in two ways: (*a*) The war may have prevented births which would have occurred among later children. Consideration of the widowhood data does not lead to the view that much weight should be attached to this. (*b*) It can be shown algebraically[1] that when, for example,

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[1] Let a be the number of third births per thousand women in a given year A. Let the ratio of fourth to third births be l, so that the number of fourth births per thousand women can be written $l.a$, where l may be equal to or less than unity. Let us suppose that all the fourth births occurring to women who had their third births in year A occur in year B. If fertility is falling yearly the third births per thousand women in year B will be less than in year A and can be written $m.a$, where m is less than unity. If the fourth births were falling off at the same rate as the third births they could be written $m.l.a$. But if they are falling off faster, the number of fourth births per thousand women in year B can be written $n.m.l.a$, where n is again less than unity. The number of births per thousand women in the two years will thus be

	Third Births	Fourth Births
Year A	a ..	$l.a$
Year B	$m.a$ ..	$n.m.l.a$

The ratio of fourth to third births given by reproduction rates would be l in year A and $n.l$ in year B. But the ratio of fourth to third births actually produced by women who had their third births in year A is $n.m.l$. Since both n and m are less than unity, $n.m.l$ is less than either l or $n.l$. Therefore under the conditions assumed the ratio of fourth to third births resulting from marriages in a given year will be less than the ratios given by the reproduction rates obtained either at the time of birth of the third or the fourth child.

rates of births of third and fourth children are decreasing but the latter is decreasing faster, the current gross reproduction rates must overestimate the ratio of fourth to third children actually produced.

The relation between gross reproduction rates and completed fertility can be looked at in either of two ways. (*a*) It may be that the fertility pattern of a marriage is largely determined at the time when it is contracted. If so, it is little affected by social changes occurring during the period of the marriage. The current gross reproduction rate in a period of declining fertility would then be made up of births at different fertility levels, the older women being more fertile and the younger women less so. The families which will be produced by the women commencing child-bearing would then be smaller than would be indicated by the current gross reproduction rate. (*b*) On the other hand, if social changes equally affect all stages in the history of a marriage, fertility of completed marriages simply represents a cross section of the gross reproduction rates over a period of time. The problem of stabilising or raising the current gross reproduction rate would then be simpler.

The ratio between second and first children is 99 per cent. complete for marriages of 1909 to 1919. This ratio is probably disturbed in the earlier years by migration and from 1915 onwards by war. Taking 1912 and 1919 as most representative of pre- and post-war marriages the ratios are 80·6 and 80·1. The uncompleted ratio for 1923 is 80·2. So that there is here no evidence of a reduction in the proportion of second children.

Another point emerges from the consideration of the ratio of second to first children. Taking the period as a whole there is a high correlation between the ratio of second to first children and the reciprocal of the absolute number of marriages, i.e., when marriages were numerous, the ratio of second to first children resulting from them was low and vice versa. This is shown in Fig. 4. The figures are incomplete from 1920 onwards. The correlation in the early years may be solely due to migration, but from 1918 onwards migration maintained a fairly steady level. In the later years the figure suggests that in a period of few marriages, those that are postponed to later years are those of older and more infertile women. The data are, of course, much too scanty to do more than suggest a possible field for investigation.

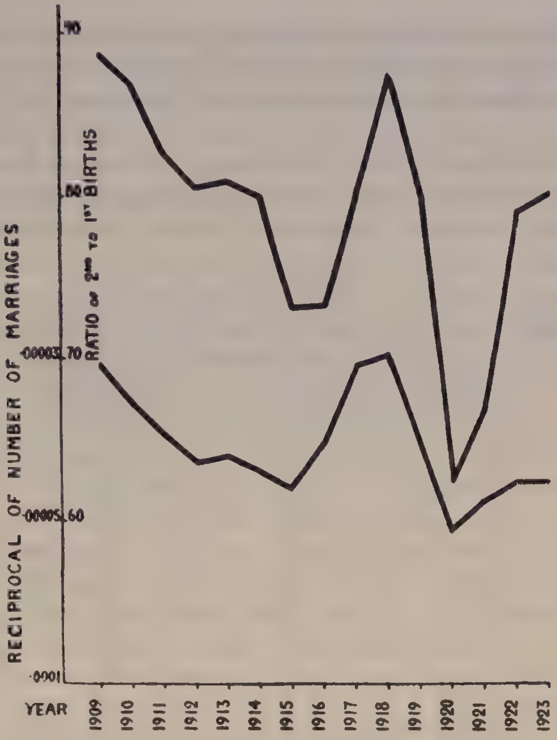

FIG. 4. The ratio of 2nd to 1st births resulting from marriages, 1909 to 1923, and the reciprocal of the absolute number of marriages in those years.

To sum up, a study of completed marriages confirms the conclusions derived from a study of reproduction rates in the two most significant conclusions (1) that the proportion of second to first children has not decreased, and (2) that the births of all later children are becoming progressively fewer.

(*d*) THE POSITION OF CHILDREN IN THE FAMILY

An increasing amount of attention is being devoted to a study of the order of birth of a child and the size of its family as correlated with intelligence and psychological adjustment. The two most important points to note in this connection are the proportion of first born to all children and of only children to all children. Including illegitimate children the proportion of first born to all children appears to have increased from about 28 per cent. in 1909 to about 34 per cent. in 1933. What is already known about the psychological and physical handicaps of the first born suggests that a further rise in this percentage might result in formidable difficulties to meet which there is urgent need for research into the uterine environment and the psychology of the only child.

The proportion of only children and of children from families of different sizes out of every 1,000 children born according to the 1911 and 1931 fertility rates is shown in Table VIII.

TABLE VIII

NUMBER OUT OF EVERY 1,000 CHILDREN BORN BELONGING TO FAMILIES OF DIFFERENT SIZES ACCORDING TO THE FERTILITY OF 1911 AND 1931

(Five-year averages)

Size of Family	1909—1913	1929—1933
1	58	65
2	75	157
3	104	155
4	117	139
5	117	113
6	104	93
7	102	72
8	80	60
9	68	48
10	61	31
11	43	24
12	29	16
13	20	12
14 and over..	24	13

The table shows that considering all children born there has been a slight increase in the proportion of only children. The most striking feature, however, is that the proportion of children from two-child families has doubled, and in 1931 was the most frequent family size, while in 1911 more children came from four-child families than from those of any other size. This is especially significant in view of the probably self perpetuating nature of the family pattern. Taking one, two and three-child families together, in 1911 they accounted for about 24 per cent. of all children born and in 1931 for about 38 per cent. In 1931, 62 per cent. of all children born still came from families of four or more, a fact which is of some importance in connection with housing, seeing that fertility in 1931 was only just about adequate to maintain a stationary population.

IV

FLUCTUATIONS IN BIRTH RATES

The last twenty-five years have seen a very large fluctuation in the birth rate due to the war, and in the last few years most countries have experienced a similar though much smaller fall and rise due to post-war depression and recovery. The most important factor in both cases was the fluctuation in the number of marriages. In Australia in 1918 the crude marriage rate was 6·65, and in 1920, 9·62. In 1931 it was 5·96 and in 1933, 7·03. With the increasing proportion of first births to which attention has already been drawn, such fluctuations have a more marked effect not only on the crude birth rate but also on the gross reproduction rate. It is of interest therefore to see whether we can detect any general trend in fertility unaffected by changes in the number of marriages from year to year.

Table IX shows the yearly reproduction rates per 1,000 women for each order of birth separately from the first to the sixth. The rates for first to third children show clearly changes due to changes in the marriage rate. The rates for third to sixth children show slight rises in 1917. As these are all in the same year midway between two censal years, they suggest some error in the proportions of older to younger women. Apart from this the fall in the rates for fifth and sixth births is continuous from 1912 onwards.

TABLE IX

YEARLY REPRODUCTION RATES ACCORDING TO ORDER OF BIRTH PER 1,000 WOMEN

Year	Order of Birth					
	1st.	2nd.	3rd.	4th.	5th.	6th.
1909	753	593	484	374	285	213
1910	756	600	490	390	296	215
1911	809	617	497	385	287	215
1912	876	658	525	396	300	220
1913	891	661	515	394	288	208
1914	881	678	509	387	280	206
1915	839	657	495	363	266	196
1916	800	652	484	355	253	184
1917	713	629	495	357	262	184
1918	653	600	481	350	250	177
1919	661	547	457	334	241	170
1920	893	586	455	334	240	169
1921	911	583	423	321	225	165
1922	836	645	432	313	225	161
1923	798	643	428	292	217	148
1924	779	626	442	290	203	144
1925	769	600	442	295	202	143
1926	755	579	426	291	191	133
1927	767	577	406	285	188	128
1928	776	570	397	275	185	124
1929	741	565	376	261	175	114
1930	731	578	362	249	168	113
1931	659	512	350	232	155	108
1932	609	494	334	218	144	101
1933	633	492	334	217	140	94

Fig. 5 shows the yearly changes in the rates for first and fourth births, together with the changes in the crude marriage-rate. The graph for fourth births is drawn to a scale twice as large as that for first births. The figure brings out both the less fluctuating character of the rate for fourth births and also its much steeper fall.

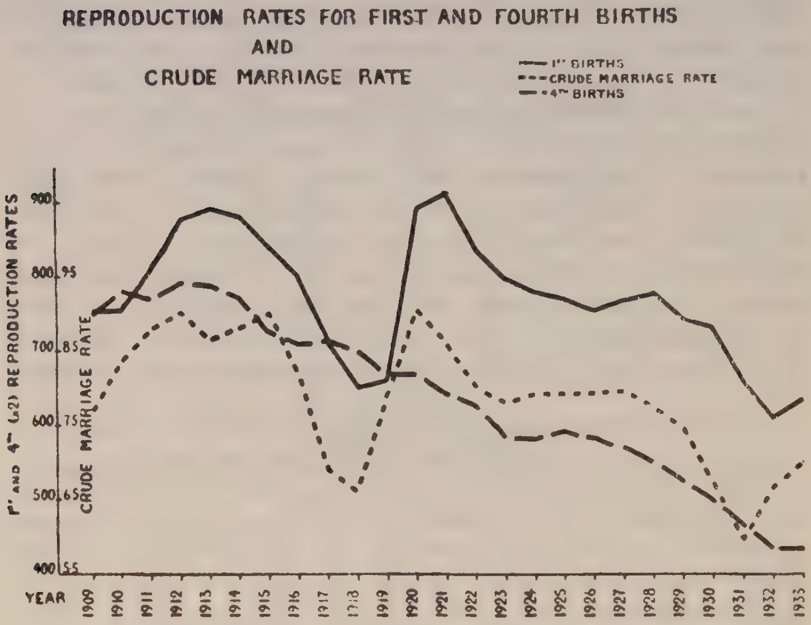

FIG. 5. Reproduction rates for first and fourth births and the crude marriage rate, 1909 to 1933.

In assessing the trend of fertility from year to year it is clearly important to be able to distinguish between first and second and later births. A very considerable rise in the crude birth rate may have no permanent significance if the rate of production of fourth or more children continues to fall. On the other hand, if the numbers of these rise, as apparently occurred in Germany in 1935, it would appear that some change in reproductive habits has taken place whether temporary or permanent.

V

The Percentage of Sterile Women

In section 3, an estimate was made of the sizes of single families without taking into account remarriages or the illegitimate children born to women who did not later marry. To obtain the percentage of childless women it is necessary to make some estimate of these two factors. Unfortunately the data do not exist for measuring these accurately for Australia. It is, however, so important to discover whether the percentage of childless women is increasing or decreasing and to what extent, that it seems advisable to make some attempt to answer it even if largely based on guess-work, in the hope that in the future statistics may become available for dealing with it adequately. The only material which throws any light on the problem is that contained in the vital statistics of Czechoslovakia for 1925 to 1927.

(*a*) Remarriage

In the Czechoslovakian statistics, the previous fertility of widows and divorcées remarrying is given. The percentage of brides who were either widows or divorcées in Australia was between 5 per cent. and 6 per cent. from 1909 to 1916, and then began to rise. The post-war peak was reached in 1922 when the percentage was 7·2. It then gradually fell, being below 6 per cent. in 1932 and 1933. The Czechoslovakian fertility percentages were applied to the first births resulting from marriages in 1911, when the percentage of remarriage was 5·4 and those resulting from the marriages of 1924, when the percentage of remarriage was 6·8. The assumption was made that the fertility of remarriages was the same as that of first marriages. On this assumption it appeared that of the births to marriages of 1911, 98·5 per cent. were actual first births, and of the births to marriages of 1924, 98 per cent. were actual first births. It was therefore assumed as a rough approximation that when the percentage of remarriage was below 6·1, 98·5 per cent., and above 6·1, 98 per cent. of first births were actually first births. In Table X column (a) gives the legitimate first births per 1,000 women together with 14 per cent. of all illegitimate births. In column (b) these figures have been corrected for remarriage by applying the percentages stated above. It should therefore give approximately the percentage of women having at least one legitimate child.

TABLE X

FIRST BIRTHS PER 1,000 WOMEN

Year	(a) First Births 14% illegitimate births	(b) First Births as (a) corrected for remarriage	(c) First Births as (b) + remaining illegitimate first births.	(d) Five-yearly averages of (c)
1909 ..	753	742	833	
1910 ..	756	745	834	
1911 ..	809	797	894	902
1912 ..	876	863	966	
1913 ..	891	877	983	946
1914 ..	881	867	967	
1915 ..	839	826	919	
1916 ..	800	788	872	
1917 ..	713	702	775	
1918 ..	653	640	709	
1919 ..	661	648	735	
1920 ..	893	875	975	
1921 ..	911	898	999	
1922 ..	836	819	906	
1923 ..	798	782	866	
1924 ..	779	763	845	854
1925 ..	769	753	834	
1926 ..	755	740	821	
1927 ..	767	751	834	828
1928 ..	776	760	845	
1929 ..	741	726	806	
1930 ..	731	716	794	
1931 ..	659	646	719	735
1932 ..	609	600	666	
1933 ..	633	623	692	

(*b*) ILLEGITIMATE BIRTHS

Account has already been taken of 14 per cent. of all the illegitimate births, which have been regarded as legitimatised after birth. To determine the total percentage of fertile women, the remaining illegitimate births must be considered. No data exist for Australia giving the percentage of illegitimate births which are first births. Australian statisticians treat all illegitimate births as first births, evidently considering the error involved in doing so to be negligible. Considering the Czechoslovakian statistics, the percentage of illegitimate births which were first births in 1925 to 1927 was 67 per cent. On examining the figures for the four different provinces separately, the percentage was found to range from 46 per cent. to 74 per cent. It appeared that the percentage of first births among illegitimate births was higher when the proportion of legitimate first births was higher, and also when the ratio of illegitimate to legitimate births was low. On these grounds then one would expect the proportion of illegitimate first births in Australia to be considerably above 67 per cent. both on the grounds of a lower illegitimate birth rate throughout the period and a lower fertility at the end of the period.

In order to combine the Czechoslovakian experience with the generally held Australian opinion an empirical formula was used to express the fact that the percentage of illegitimate first births varies in direct proportion to (a) the percentage of legitimate first births, and inversely as (b) the illegitimate birth rate. If x is the percentage of first births to all legitimate births and y is the percentage of illegitimate births to all births then $3x—2y$ is taken to be the percentage of first births among illegitimate births. This formula fitted the Czechoslovakian data fairly well, and applied to the Australian data gave percentages of illegitimate first births ranging from 61 per cent. to 86 per cent. Column (c) in Table X shows the figures obtained by applying the formula and is therefore a rough estimate of the number of women in every thousand having at least one child in the course of their lives on the basis of the fertility of the given year.

In column (d) five-yearly averages for the years centring on 1911, 1913, 1924, 1927 and 1931 are given. The figure for 1913 is probably greatly affected by migration. Taking the 1911 figure as more representative of the pre-war period, it appears that the proportion of fertile women has fallen in twenty years from about 90 per cent. to about 74 per cent. It might be well to recapitulate the principal sources of error involved in this estimate. The first is in assuming that the fertility of remarriages and first marriages is the same. Although no data on the subject are known to the writer it seems likely that remarriages are probably less fertile. There is a further error in assuming that the previous fertility of the remarried is accurately represented by the Czechoslovakian fertility of 1926. It was probably greater than this at the beginning of the period and less at the end. This source of error would lead to an overestimate of fertility at the beginning and an underestimate at the end of the period, but the amount of this error cannot be very great. Finally it might appear that fertility has been underestimated by taking too low a percentage of illegitimate births. Thus both the principal sources of error would lead to an underestimate of fertility. On the other hand there seems no reason to think that they would falsify the trend from 1909 to 1933. Moreover any higher estimate of fertility would seem, even allowing for the effects of migration, to lead to improbably high figures in the earlier part of the period.

The error due to women whose illegitimate children die and who later marry and have a legitimate child recorded as a first birth has been dealt with in §3. It was there shown that this source of error would not be likely to reduce the percentage of fertile women by more than 1 per cent. in the pre-war period and by considerably less in the post-war period. There still remains to be considered the women who have illegitimate children by other fathers than those they later marry. It is impossible to make any guess at the number of these women. On balance the errors enumerated in this paragraph appear to tend in the opposite direction to those enumerated in the last paragraph, so that there does not appear to be any reason to alter the figures given.

The figures given in column (*c*) for 1920 and 1921 call for some comment. It is obviously impossible that at any time or place 100 per cent. of the women in a community would be fertile. There is no significant error in the numbers of women on whom the rate is based, since 1921 was a Census year. Nor is any significantly different result obtained by computing the gross reproduction rate by single years instead of by five-yearly age groups. The total number of first births per 1,000 women obtained for 1921, working with single years of age, was 998. Clearly the result is due to the accumulation of postponed births. Gross reproduction rates express current fertility in terms of the total fertility of women over the whole reproductive period. When fertility is such that it could not possibly continue as in 1921, gross reproduction rates like all other indices of fertility cease to have any significance as measures of total fertility.

If the figures given in Table X are accepted as a rough estimate, it is possible to make some estimate of the relative weight to be attached respectively to increasing sterility and decreasing size of family. In 1911 the gross reproduction rate was 1·69 and in 1931 it was 1·13. If the family sizes of 1911 were associated with the 1931 percentage of sterility a gross reproduction rate of 1·38 would ensue. The same gross reproduction rate would be obtained by associating the family sizes of 1931 with the sterility of

1911. It would therefore seem that increasing sterility and decrease in the number of large families were about equally important during these twenty years.

A further point merits consideration. In the light of what was said in section 3 about the relation between the gross reproduction rate and completed families, it seems probable that although the 1931 gross reproduction rate was sufficiently high to maintain a stationary population, the proportion of fertile women was already too low for this. In view of the attitude of present-day communities to large families it would appear likely that when the proportion of sterile women rises above 25 per cent. fertility will fall below the level adequate to maintain a stationary population.

VI

FERTILITY OF MARRIAGE

In order to arrive at an estimate of the proportion of fertile marriages on the lines of the work hitherto recorded it would be necessary to determine the proportion of women who marry at least once before reaching the age of fifty. The numbers recorded at the Census as never married in the age group 50—54 have increased steadily from 6 per cent. in 1891 to 16 per cent. in 1921. These figures reflect the marriage rates over the period of thirty-five years previous to the Census. The crude marriage rate has fluctuated widely since 1860 without displaying any very marked fall. It is possible that the consistent trend of the Census figures above quoted may be illusory in that the high percentage of never-married in 1921 may reflect the low crude marriage rate in the period 1892 to 1898, when it was below 7 per 1,000. The rate did not fall below 7 again until the war years and again in 1930. It is possible to arrive at a percentage of never-married based on the rate at which women are marrying in a particular year by constructing a nuptiality table based on the same principles as a life table. The numbers of single and married women for each age group must be known, and in Australia these are only available for the Census years. Unfortunately the 1933 data are not yet available and both 1911 and 1921 were years with an exceptionally high marriage rate. Nuptiality tables calculated for these years give a percentage of never-married at exactly fifty years of age of 12 per cent. and 10 per cent. respectively. In the absence of more data it is only possible to make a guess at the true marriage rates corresponding to the gross reproduction rates of 1911 and 1931. It seems most likely that the nuptiality table giving 12 per cent. of never-married women at fifty for 1911 may represent the true rate for that period and that the 1921 Census figure of 16 per cent. may represent the rate for the 1931 period. Accepting these figures the conclusions arrived at are presented in Table XI.

TABLE XI

PERCENTAGES OF FERTILE AND STERILE MARRIED WOMEN PER 1,000 WOMEN

Year	Married Women	Fertile	Sterile
1911 ..	88	80.5	7·5
1931 ..	84	66	18

From the figures in Table XI it appears that 91·5 per cent. of all marriages in the 1911 period were fertile and 78·5 per cent. of all marriages in the 1931 period. While these figures can be advanced with very little assurance they can, to some extent, be corroborated or otherwise by a study of the fertility of completed marriages on the lines referred to in section 3. As far as could be ascertained the mean fertility of marriages contracted in the five years 1909 to 1913 was 88 per cent. For the three years 1924, 1925 and 1926, fertility during the first seven years after marriage could be studied. This period covers 98 per cent. of all first births. 79 per cent. of the marriages contracted in these three years were found to be fertile, probably corresponding to a total fertility of about 80 per cent. The study of the fertility of completed marriages therefore agrees fairly well with the figures arrived at from a study of current data. The final conclusion then is that in the 1911 period about 90 per cent. of all marriages were fertile and at the present time probably rather less than 80 per cent. are fertile.

VII

Age of Women at Marriage and Birth of Children

It is generally believed that declining fertility is associated with a postponement of marriage and childbearing. In the Australian period studied this has not been so. Table XII gives the mean age at marriage, at birth of the first child, and at the birth of all children. The figures are higher in 1921 for all three on account of the postponement of marriages and births during the war. There is a very slight rise in the age at marriage. This clearly affects chiefly the more infertile marriages since the mean age of the mother shows a slight fall over the twenty-five year period.

Table XII

MEAN AGE OF WOMEN AT MARRIAGE AND BIRTH OF CHILDREN

Year	Mean age at marriage	Mean age at birth of 1st child	Mean age at birth of all children
1909	25–2	25.4	29.4
1912	25.2	25.4	29.2
1921	26.2	25.8	29.3
1931	25.3	25.1	28.8
1933	25.4	25.0	28.3

While there has been some tendency to the postponement of marriages and births among older women, this has been more than counterbalanced by the high marriage rate and high fertility of the younger women. Fig. 6 shows the distribution of ages at marriage in 1909 and 1933. It is seen that the 15—20 age group constitutes a larger proportion of all brides in 1933 than in 1909. The percentage of marriages of minors is given in the official statistics as 20·5 in 1909 and 23·4 in 1933. There are also more marriages among the 40—50 age group so that the average remains practically the same. Fig. 7 which gives the distribution of ages at birth of the first child shows the same phenomena, greater frequency in the youngest age-group with some tendency to postponement of births to a later age. Since the fertility of the later age groups is relatively insignificant, the net effect is a reduction of the mean age.

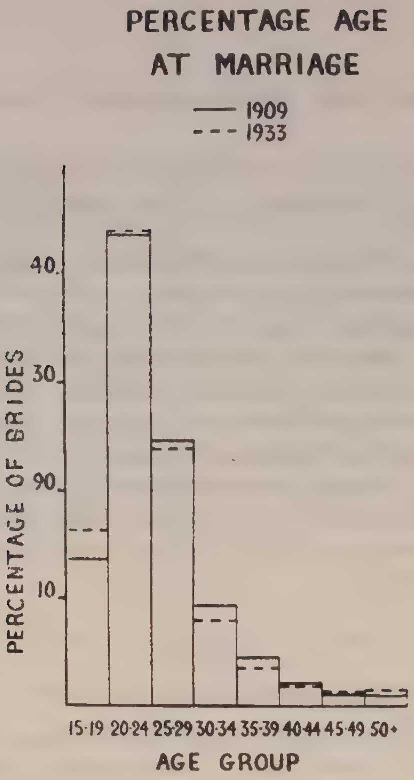
PERCENTAGE AGE AT MARRIAGE

FIG. 6. Percentages of Brides in different age groups, 1909 and 1933.

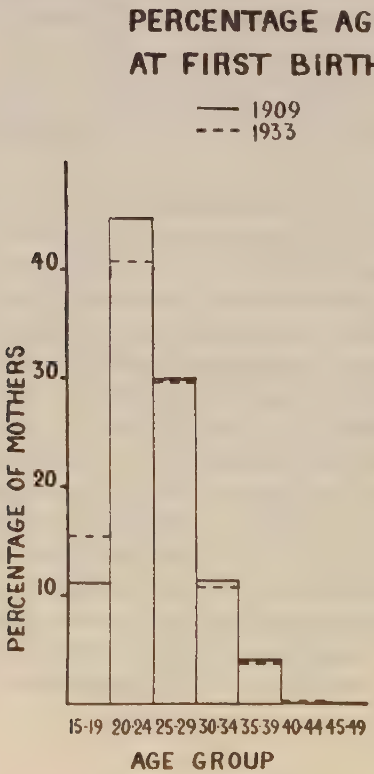
PERCENTAGE AGE AT FIRST BIRTH

FIG. 7. Percentages of Mothers in different age groups at birth of first child, 1909 and 1933.

The same tendency to a bimodal distribution is seen when the period elapsing between marriage and the birth of the first child is considered. The mean length of the period was 15·2 months in 1909 and 19·4 months in 1933. Fig. 8 gives the distribution in these two years. The increase in the numbers born less than nine months after marriage has to some extent replaced a number of illegitimate births. The increase is so marked that in 1931 to 1933 more first children were born in the sixth month after marriage than in any other single month. Considering the post-nuptial conception alone there has been a more considerable increase in the period elapsing between marriage and birth than is reflected in the mean duration.

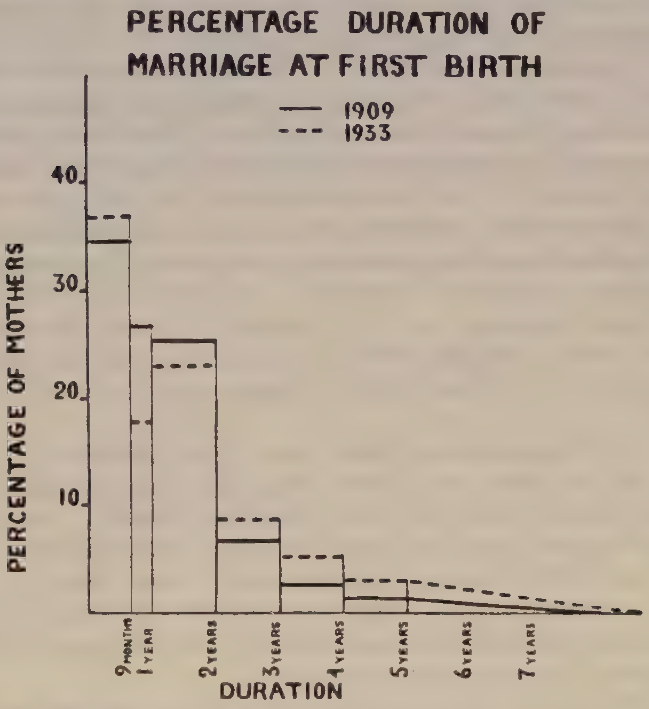

Fig. 8. Percentage of Mothers of first children at different durations of marriage, 1909 and 1933.

VIII

SUMMARY

(i) The particulars elucidated at birth registration in Australia since 1909 make it possible to estimate within certain limits the changing distribution of families of a given size. It is thus possible to infer the contribution of (*a*) an increasing proportion of marriages which have no issue and (*b*) diminution of large familes towards declining fertility.

(ii) The gross reproduction rate was calculated for the years 1909 to 1933 inclusive. It was 1·78 in 1912 and 1·05 in 1933. There was thus a 41 per cent. fall in fertility in twenty-one years. The fall was more marked in illegitimate than in legitimate fertility and greater in the older age groups than in the younger.

(iii) Tables were presented giving the distribution of sizes of families resulting from current marriages. It was found that over the period 1911 to 1931 (*a*) the proportion of sterile marriages had considerably increased, (*b*) there was little change in the proportion of one child families, (*c*) there was a considerable increase in the proportion of two-child families, and (*d*) there was a reduction in the proportion of all families of four or more children. The result of these changes was to make two children the fashionable family size.

(iv) The results obtained were compared with the fertility of completed marriages of 1909 to 1911. This led to the conclusion that the completed families resulting from marriages contracted at the present time are likely to be smaller than is indicated by the current gross reproduction rate even if no further social changes reducing fertility take place.

(v) The proportion of first born to all children has risen from 28 per cent. to 34 per cent. In 1909 to 1913 more children came from four and five-child families than from any other size, but in 1929 to 1933 more children came from two-child families than from any other size. In 1929 to 1933 when the gross reproduction rate was just sufficient to maintain a stationary population 62 per cent. of all children came from families of four or more.

(vi) The crude birth rate and also the gross reproduction rate has fluctuated widely during the last twenty-five years. These fluctuations are accentuated by the increasing proportion of first births. The reproduction rates for fourth and later births have been much more steady. Hence they form a better index of the general trend of fertility.

(vii) The proportion of sterile women (using the term in a purely statistical sense) was estimated at 10 per cent. in

1909 to 1913 and 26·5 per cent. in 1929 to 1933. The latter percentage is probably incompatible with the maintenance of a stationary population so long as the present attitude to large families persists.

(viii) The fall in fertility between 1912 and 1933 appears to have been due in about equal measure to the increasing proportion of sterile women and the reduction in the numbers of large families.

(ix) The fall in fertility has not been accompanied by any increase in the mean age at marriage or at childbirth. The tendency to later marriages and postponement of birth among older women has been more than counterbalanced by the relatively better maintained fertility of younger women.

ACKNOWLEDGMENTS

The author is greatly indebted to Professor Lancelot Hogben, F.R.S. and to Dr. Kuczynski for suggestions and criticism and to Miss Frost for assistance in computation. Thanks are also due to the officials of the Intelligence Branch of the office of the High Commissioner for Australia for their courtesy in supplying information and literature.

Some Theoretical Aspects of the Gold Problem[1]

By T. Balogh

I

A great deal of most ingenious and elaborate statistical speculation has been applied to find a theoretical foundation for the solution of the problem as to whether economic stability is compatible with the maintenance of gold as the basis of the world's monetary system. The analysis of the essential rôle of gold and of the position of the gold producing industry in the economic system has receded into the background. A rather mechanical view has grown up. Certain stereotyped calculations are performed, mostly comparing economic progress, a very nebulous concept, with the rate of increase of the gold base—a concept whose apparent simplicity tends to disappear on an analysis of its deeper economic significance.[2] I do not propose to follow this line of approach, because it will become evident from the considerations below that this method can hardly furnish a clear answer to the gold problem, even when most countries are on the gold standard. It appears to be almost useless when, as at present, the automatic gold mechanism has been abandoned by most countries.

More hopeful, perhaps, would be an attempt to reformulate the economic dynamics of the process of absorption of gold, especially newly produced gold, by the system, and the consequences of this process. We shall try to discover the reactions to a change in the effective volume of the monetary gold stock, first in a closed economic system and then in a world of interdependent national economies. Both will have to be analysed under the assumption first of full employment of productive factors, and then of unemployment. Having determined the causal relationships, we shall have to discover the actual order of magnitude of the factors found to be relevant, as quantitative differences may well have a decisive influence on the final effect, and therefore on our conclusions.

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[1] The basic argument was prepared for two lectures before the Hungarian Economic Society and the Royal Institute of International Affairs in May and June. Some of the latest publications could, therefore, not be utilised.

[2] The Reports of the B.I.S., to which constant reference has to be made in all matters concerning current data, provide brilliant exceptions to this generalisation.

We shall then be able to evaluate the rôle of gold in the present business cycle—especially in promoting recovery—and the probable part it will play, if present trends continue, in the future. These conclusions will also provide a basis for choosing among the possible remedial measures—if any should be found necessary. It will be assumed throughout this paper that the maintenance of the gold standard in some form is desirable; a proposition which seems still generally to be accepted.

II

Gold in a Closed Economic System

The basic assumption in the first stage of our analysis is a closed system. We shall consider a modern gold bullion standard. Gold is the base of the credit structure. Payments are effected by means of cheques and banknotes. Gold is used outside the banking system merely for hoarding purposes.[1]

The essence of the gold standard is that gold can be used as money, or rather exchanged into money at a fixed rate. This fact implies that the elasticity of demand at the prevailing price is infinite, and that, therefore, fluctuations in the effective amount of purchasing power will follow changes in gold output.

The different forms or stages in the evolution of monetary arrangements merely alter the quantitative limits of this phenomenon. Under the primitive gold coin standard the limit in the change of the volume of money is given by the change in the supply of monetary gold itself.[2] In a system where banknotes are used, the impact will be identical if full gold cover is required against notes. But if this is not so the impact effect will be modified either in the same direction as the original alteration of the gold stock or in the opposite direction, thus mitigating or intensifying the effect in the change of volume of gold. In an economy with a fully developed banking system the intensification[1] of the primary change is further increased by the fact that banks keep their—proportionate—reserves with the Central Bank, which in its turn has to cover only a given proportion of them by gold.

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[1] If gold is used not as money but merely as an ornament or store of value the position of the industry is identical with that of other industries producing goods of extreme durability, e.g., diamonds, which have received ample treatment.

[2] Cf. for all these problems the brilliant analysis of Professor Viner, *Studies in the Theory of International Trade*, London, 1937, pp. 290–436.

In a primitive gold standard system the demand for any addition to the existing stock of gold is derived from that part of the income which is not used to buy other commodities, whether consumption or production goods. In equilibrium the change in the cash reserves or in the use of gold for ornament must be exactly equal to new production, for otherwise either additional purchasing power would be created or a deficiency in total monetary demand would arise. We may formulate this condition by saying that in equilibrium the production must be equal to the demand for the increase of cash balances (and hoards) as compared with the preceding period, i.e., must be equal to savings minus voluntary investment in capital goods other than gold. In the case of a banking system with proportionate gold reserve requirements, this conclusion must be modified. The increase in gold production will lead to inflation if, given the prevailing banking regulations and practices, it brings about an increase in bank money greater than that part of current savings which people want to add to their cash holdings.

There is, of course, no " deflation " involved in this quasi gold " hoarding "—as the " savings " used are paid out as costs of production and profits and so swell the general circulation. This conclusion will be found useful when we come to analyse the difference between the economic consequences of *a*, a change in the value of existing gold stocks, and, *b*, a change in the value of the annual output.

If additional purchasing power is put into circulation, i.e., if gold production exceeds these limits, then a cumulative inflationary movement in prices and incomes will start and will continue until the national money income has risen to such an extent as to fulfil this condition.[2]

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[1] Or the reverse. The increase in the reserve ratios would " sterilise " the increase in the volume of gold. Cf. below, p. 279.

[2] The fall in gold production resulting from the increase in prices and costs will help to re-establish equilibrium. See Cassel, *Theoretische Nationaloekonomie*, Leipzig, 1923, pp. 433–437.

The impetus of the cumulative process is, therefore, a function of the ratio of gold production to the current flow of savings as well as of the ratio of the increase in incomes to the increase in cash reserves and of the existing banking practices and regulations.

In the course of the cumulative process anticipations will probably be aroused which will inevitably be doomed to disappointment once these limits are reached. The position established as a result of a change in the supply of gold will, therefore, probably be unstable. A reverse process may start in consequence of the errors committed. It is probable that the extent of these errors, and therefore of the subsequent reaction, will vary with the intensity of the boom. The continued injection of additional purchasing power during the ensuing depression as a result of increased gold production will, however, mitigate its force. The final outcome of the fluctuations thus engendered can therefore not be predicted. Little can be said also about how the changes in gold production modify the ebb and flow of the business cycle caused by other factors.[1] These phenomena, and especially the problems of the upper turning point, are well known and need not be restated.[2]

There is a kernel of truth in the widely publicised attempts to explain movements in the price level by fluctuations in the gold production above or below the mystical 3 per cent.[3] If the prevalent monetary system is the gold standard and if the increase in cash requirements—leaving aside cyclical fluctuations—is fairly steady, then that very empirical law has some solid foundations. Mr. Hawtrey has given a formidable array of reasons why the empirical basis (1850–1910) chosen by the statistical experts robs their conclusions and predictions of all economic relevance.[1]

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[1] See Mr. Keynes, *The General Theory of Employment, Interest and Money*, London, 1936. and Mr. Harrod, *The Trade Cycle*, Oxford, 1936. Mr. Keynes put new emphasis on the effect of an increase of national money income on savings and thus on the problem of the re-establishment of equilibrium. Present investigation and the concepts used are, however, based on the Robertson–Swedish–Austrian (especially Professor Haberler) period analysis. The multiplier which is implicitly used is the *ex-post* multiplier which does not suffer from the shortcoming of the neo-Cambridge school in excluding the capital "deepening" phenomenon in the cumulative process.

[2] Cf. the brilliant summaries of the development of thought in Haberler, *Prosperity and Depression*, London, 1937. Viner, *op. cit.*, esp. Chaps. V and VI, and in Professor Hayek, *op. cit.*, Chap I.

[3] The literature concerning this problem is as limitless as it is tedious. Argument after argument is brought up again and again. The most important documents are: Cassel, *op. cit.*; League of Nations Gold Delegation of the Financial Committee, *Interim Report*, Geneva, 1930; Warren and Pearson, *Gold Prices*, and the very thorough analysis of Professor Hardy, *Is There Enough Gold?*, Washington, 1936. The appendix of this last publication, written by Mr. Tucker, seems to fall into the opposite error by denying that gold production had any effect on prices after the Californian discoveries. L. Edie, *Gold Production and Prices*, Indiana, 1928.

We have established the conditions of equilibrium with respect to gold production. We must now investigate certain relevant cases of disequilibrium.

(1) Suppose that the increase in gold production takes place in an economic system which is under the influence of deflation or contains unemployed resources. The deflation will then be *tanto quanto* offset, production increased, by the expansion of gold production. As the increase of gold production has, *ceteris paribus*, the effect of increasing the demand for capital, it will act as a shock absorber in case of crisis, a shock absorber far more powerful than open market operations, because it acts not merely on the supply side of the capital market (by increasing the bank reserves or volume of currency) but also on the demand side where help is most needed. It creates incomes and can under favourable circumstances reverse the cumulative deflationary spiral. In these conditions alone can the increased gold production take place without a parallel shrinkage of other activities. This conclusion is important when we have to consider the methods of dealing with the present gold problem.

(2) If gold production increases above the defined equilibrium level and the economic system is fully employed, an inflationary spiral can only be avoided by a fall in either consumption or investment. The authorities must artificially induce hoarding either by decreasing the expected rate of profit, and therefore of investment and incidentally consumption (e.g., by evoking a crisis of confidence)[2] or by appropriating current income for the purpose of buying gold, thus acting both on consumption and investment, or by forcing investment in gold, or by borrowing current savings for its purchase. It is these two latter methods which have been chosen in most countries.

(*a*) The forced investment in gold has been accomplished by the increase in reserve requirements of the banking system or the reduction of the fiduciary issue or a sale of open market assets of the Central Bank. This induces, *ceteris paribus*, a sale of assets by the banking system, and therefore a readjustment of the market rates of interest to the new equilibrium : current savings are used to buy these securities instead of new issues. The supply of savings for other purposes diminishes.

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[1] See also Royal Institute of International Affairs, *The International Gold Problem*, London, 1931: "Discussion on Mr. Kitchin's Paper," pp. 56–83. We may add that the rhythm of technical progress is alone sufficient to falsify the method. Lower prices from quickened technical progress would not imply disequilibrium but the reverse.

[2] This method was adopted recently both here and in America.

(*b*) The neutralisation of the excess gold production by creation of debt has been accomplished by the institution of so-called Exchange Equalisation Funds.[1] Fashionable and new as they appear to the public and even to some experts, they do not represent any advance on earlier methods,[2] such as the acceptance of interest-bearing deposits, segregated from the " reserve accounts ", by the Central Bank. Their one important function is to permit restrictive open market operations even if all the assets of the Central Bank other than gold have been sold. They are, incidentally, an expedient for transferring the cost of absorbing gold from the member banks (who have to bear it if their reserve requirements are raised as a result of the necessary sale or reduction of interest bearing assets) or the Central Bank (sales of open market assets which yield income) to the Exchequer.[3]

The working of the sterilisation process is well known and need be restated only very briefly. The sale of gold[4] to the Central Bank or the Treasury by the producer increases the reserves of member banks by the amount of the cheque or notes which the producer receives in payment and deposits with his bank. The deposits and the cash reserves of the member banks thus increase. There arises a danger of multiple expansion. The Treasury therefore sells an equivalent amount of Treasury bills[5] and withdraws the proceeds from the reserve of the member banks. As reserves will have shrunk back to the original level, but deposits and assets will still be increased by the amount of the gold sold, the member banks will be forced—provided they do not let their reserve ratio drop—to liquidate some assets and thereby extinguish deposits.[1] The first expansion will be followed by an equal contraction. The original position, however, will not thereby be re-established. The amount of Treasury bills outstanding has increased and must be carried by the original amount of deposits. If the velocity of circulation does not increase, and there is no *a priori* reason why it should, interest rates will be forced upwards.[2] The necessary extent and therefore the cost of sterilisation will increase, *ceteris paribus*, proportionately with the refinement of the credit mechanism and inversely with the reserve requirements. The variation in the cost of sterilisation is a reflection of the fact that, under more developed currency arrangements, the real cost as measured by the amount of factors devoted to gold mining is smaller. The difference between the minimum gold production necessary to support the monetary system and a given gold production is proportionate to the extent of the substitution of bank money for gold.

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[1] Cf. N. F. Hall, *The Exchange Equalisation Account*, London, 1935.

[2] Except that secrecy is obtained. After an acute disturbance of confidence and as long as the reserve position is weak this may be an advantage. It can outlive and, in the case of England, has long ago outlived, its usefulness and may politically be very harmful. The increased publicity now vouchsafed is therefore welcome. Cf. Keynes, *The Times*, June, 1937.

[3] In England in the latter case no such transfer takes place as the profits and losses of the Issue Department are born by the Exchequer.

[4] The process is identical if the sale is of foreign currency which the Treasury afterwards converts into gold.

[5] If gold is sold directly against Treasury bills the initial increase in bank reserves and deposits will not occur. But there will still be an increase in the outstanding debt of the Government.

It seems worth while to emphasise that this increase in the rate of interest is *not deflationary* : the purchase price for gold is not hoarded but spent by the recipients. *If* gold production is higher than necessary for the voluntary increase in cash holdings and its *inflationary* effect on the price and income structure is to be prevented, then the rate of interest must rise. The rise in the rate of interest is not a sign of primary deflation, but is necessary to avoid inflation. It may, of course, happen that its repercussions on the Stock Exchange and on general confidence originate a genuine deflation.

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[1] The process will first affect single banks and will be diffused through the system by appropriate changes of the policy of these banks and their effects on their clearing settlements.

[2] The authorities may, of course, avoid this effect by leaving such part of the original rise in cash reserves of member banks as will allow for an expansion of credit just sufficient to carry this increase of the Treasury bill circulation. We may call this procedure a sterilisation of the multiple credit expansive effect of gold. The cost of this partial sterilisation will vary inversely with the reserve ratio of member banks. There will thus be no primary effect on the rates of interest previously ruling (the secondary effect via the cumulative process may be to increase them). But there will be an expansion of the volume of bank money equal to the increment to gold stocks. This will engender a cumulative expansion though of much less impetus. In May, e.g., last year the short term rates stiffened considerably as a result of the influx of French funds. The Exchange Equalisation Fund thereupon sold gold to the Bank and permitted an increase in the cash reserves of the banking system by repaying Treasury bills. Another method is open market purchase of securities by the Central Bank.

To conclude our investigation of a closed system, we may point to the difference between an increase in the value of gold production and an increase in the value of the existing gold stocks. In the former case demand for capital is stimulated and the monetary circulation increased *pari passu* with the increase of production. The increase in the value of the existing gold stocks in terms of money is economically only significant if that profit is spent, i.e., if the Government or the individuals to whom the profit accrues put it into circulation. It then increases incomes and the credit base. Otherwise it merely serves to increase the inactive gold reserve of the country or the reserve ratio of the banking system, and is *ipso facto* sterilised. There is, therefore, no economic significance in the comparison of the value of gold stocks before and after a change in the price has taken place, except under the assumption that those to whom it accrues (in practice the Governments) will use the profit as revenue.

An increase in gold production, we find, reduces, at full employment, the amount of factors available for other purposes. In this sense it reduces maximum real income which would otherwise be attainable for the community. This finds its expression in a higher level of interest rates than would otherwise obtain.

Variations in the output of gold play unquestionably only a secondary rôle in the causation of economic fluctuations. If, however, a considerable increase in gold production takes place during the preceding period of slump, the strain imposed on the system near full employment—if inflation is to be avoided—may be considerable, i.e., the rise in the rates of interest very sharp, and its repercussions deleterious. A compromise between unrestricted use of the increments to the gold stock and their complete sterilisation will require very great skill. In earlier periods no attempt was made to arrive at a rational gold policy. We therefore see, in periods of large gold output, low short term rates of interest, followed by booms, which eventually raise the rates of interest to a higher level. In spite of these added difficulties it seems that in an economic system in which unemployment, and consequent loss of potential real income, is the rule rather than the exception, the maintenance of a plentiful gold supply may prove advantageous, providing a constant stimulus.

III

PROBLEMS OF INTERCONNECTED ECONOMIC SYSTEMS

The problems of the monetary mechanism of international trade payments have been so exhaustively discussed recently that only certain relevant implications need be dealt with here.[1] The condition of stability is more complicated: not only must each economy be in equilibrium in accordance with the condition above, but the condition must apply to the system as a whole.

(1) The usual assumption in earlier writings of the rôle of gold in an interconnected system of national economies, all of which have a statutory price for gold, which found its most extreme exponent in M. Rueff,[2] is that the effective money in circulation remains constant when a transfer of specie takes place within the system. It has lately come to be recognised that this hypothesis cannot be generally entertained. There are two main reasons why an interconnected system shows different reactions from a closed economy[3]:

(i) the efficiency of monetary arrangements may be different in different countries. If so, the transfer of a given payment to the country with a less (more) developed monetary system will result in a deflation (inflation) in the system as a whole. There will be net "hoarding" ("dishoarding") in the system as a whole, even without a change in the volume of specie.[4]

(ii) the transfer of specie can start a cumulative process in either of the countries which may extend its effects into the other instead of causing an offsetting reaction. The export of specie, therefore, may increase employment in the system as a whole. A country which already has full employment thus may increase its real income by exporting part of its gold reserve until all other countries also have full employment. The import of specie will cause an increase in total demand in the receiving country and its employment will increase. In the same manner a deflationary shock can also be transmitted.

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[1] See Viner and Haberler, *loc. cit.*, also Ohlin, *Interregional and International Trade*, and C. Iversen, *Aspects of the Theory of Capital Movements*, 1935.

[2] Mr. Keynes' views on the Transfer problem. *Economic Journal*, 1929.

[3] Cf. Viner and Haberler, *loc. cit.*, also my paper, "Some Theoretical Aspects of the Central European Credit and Transfer Crisis," *International Affairs*, Vol. II, May, 1932.

[4] See Viner, *loc. cit.* An extreme case for an inflationary effect is the establishment of gold exchange standard. The same gold begins to support two credit structures. The reverse happens when the gold standard is introduced to a country previously having had other monetary arrangements (including gold exchange standard).

(2) Let us suppose that two countries have identical monetary arrangements.[1] Specie moves between them because of an excess of international payments either on income or on capital account. The gold import may have a direct effect on demand or merely an indirect one through a change of the credit position.

If the inflow is a result of an increased value of exports there must necessarily have been an increase in the total demand for the goods of that country.[2] The effects will depend on the state of the partial equilibrium or disequilibrium in the country receiving the specie and on the character of its monetary system.[3]

If the import of gold is a consequence of a capital movement its effect will depend on what use the owner[4] makes of the national currency thus obtained and on the currency arrangements of the receiving country. If he spends or invests it, total money demand obviously increases. If he desires to increase his bank balance, the internal money demand for goods will remain unchanged. The volume of deposits is indeed increased, but the average velocity of circulation falls. There is an increase in the base of credit which enables the banks to increase their deposits. Their willingness to lend therefore is increased, the rate of interest has a tendency to fall and a generally expansionist atmosphere is created. Sterilisation can completely offset the effects of the import of gold but must stiffen both the short and long term rates of interest.

The purchase of securities with imported funds increases their price. Additional purchasing power is put into the economic system by way of increased profits on the Stock Exchange or an increase in the capital available for new flotations. The deposits created are thereby transferred to the seller of the security. The reserves of the banking system increase. If this increase is sterilised and the reserves reduced to the original level the banks must either sell securities or withdraw credit. The effect of the influx on Stock Exchange prices or consumption is not certain to be neutralised. It may happen that a deflationary pressure of equal force will be exercised on the other parts of the economic structure. The process is the equivalent to what happens if an internal creditor purchases securities and pays for them by selling fixed interest securities or by calling in a loan. In America, where speculative movements are of great force, and the basic atmosphere bullish, this process causes disequilibrium by distorting the structure of interest rates.[1]

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[1] This assumption is merely for the sake of simplicity. Its dropping does not modify the relevant conclusions though it causes complications.

[2] It may well be the result of the turning of the terms of trade in its favour.

[3] Another case which need not be analysed in this connection as it is of no practical importance for our present problem, is the import of specie because of a fall in imports.

[4] It may be the result of repatriation of funds hitherto invested or kept abroad. It may be the consequence of borrowing abroad or it may be flight money seeking investment in the country.

(3) If gold production is carried on in only one of the two countries which form the system, say country A, an increase in gold output leads to an increase of productivity in that country, and, if monetary equilibrium existed previously, may bring about inflation if its rates of interest do not rise.

(i) If the gold output is not sterilised in country A incomes increase there, and with them the demand for imported goods. At the same time the increase in the profit rate induces capital imports. Both will tend to equalise the rate of profit in the two countries and to increase national incomes to the extent required by the conditions of the new equilibrium. The whole system, therefore, will undergo inflation.

(ii) If the non-gold-producing country B attempts a sterilisation of the gold influx caused by the increase in its exports consequent on the increased demand for goods in A, the inflation of the whole system will be smaller. Equilibrium will be established at a smaller national money income in country B with a higher rate of interest in the period of impact. The burden of carrying gold will be put primarily on B.

(iii) Finally, let us suppose that the gold producing country A has no statutory price for gold and that country B is on the gold standard and equilibrium exists. An increase in the gold production in these circumstances will lead to an increase of gold imports into country B. Its currency, therefore, will depreciate in terms of the currency of A until an export surplus, of an amount exactly equal to the increase in gold production, develops[2] in country B as a consequence of the increase in the purchasing power of country A for B's products (even though national income in terms of currency A need not have risen as gold is not freely exchangeable into that currency).[1] The depreciation of B's currency concomitantly reduces the price of gold in A and therefore helps to re-establish equilibrium by curtailing production and lessening the divergence between the new and old equilibrium rates of return on capital. It will probably also attract capital from country A, as the inflationary process thus induced gives rise to windfall gains. This tendency hinders the establishment of final equilibrium as it lessens the depreciation of B's currency. The influx of gold will also create a price movement in country B tending to re-establish *internal* monetary equilibrium via increased national income. This, too, will tend to retard the establishment of the new *international* equilibrium and to increase its divergence from the old: the fall of the value of both gold and B's currency will have to be larger, as the increase in internal prices and incomes in terms of currency B will have to be more than offset. If B sterilises the imported gold, its rate of interest must increase. The consumption of, and investment in, goods other than gold will decrease in country B provided that in the initial state full employment obtained. If gold is not sterilised, the shift will be accomplished by a decrease of imports and increase of exports. If sterilisation is attempted, the decrease of home investment will form part of the readjustment. In the absence of frictions this shift will not entail primary deflation in the system as a whole. This fact will provide but small consolation to those marginal entrepreneurs and workers who will be temporarily squeezed out. The effect will be politically the more dangerous as it originates abroad.

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[1] The dangerous implication of this disturbing effect of a Stock Exchange boom has been investigated in my paper, "Latente Inflation", *Schmollers Jahrbücher*, 1929.

[2] The steps of this process, shifts between home and foreign goods in both countries, need not be discussed here.

During the period of transition true deflation might originate if sterilisation is attempted, owing to the repercussions of the increase in the rate of interest on the capital market, and it may extend to country A through the increased competition of B's exports.

If in the original position there was an unemployment of productive factors in B, increased gold imports and the depreciation of the currency will tend to mitigate it. The consequences of a sterilisation need not be discussed in this connection.

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[1] The depreciation of the currency of (B) and consequent increase in its export may well create a deflationary spiral in (A). If e.g., the dollar had depreciated merely as a result of the refusal of Britain to purchase gold, this effect might well have been formidable.

A further case must be discussed finally, similar to the case of capital movements induced by the depreciation of B's currency. It has, however, lately acquired special significance. The sterilisation of the gold imported by B causes the level of its rates of interest to stiffen. They may move above those ruling in A. If no further depreciation is expected, or, *a fortiori*, if an expected appreciation induces forward buying and enables banks in A to lend without incurring exchange risk, the gold movement into B will be effected without further depreciation. The basic disturbance, the increase in the gold production, is not eliminated. The sterilisation will, to use a contemporary expression, *create* the "hot" money which, on the basis of an only superficial study of the balance of payments, is blamed for the original gold import. The continuation of gold imports into U.S. in a period when the dollar is declining and the simultaneous persistence of the premium on forward dollars suggests that this factor must have played an appreciable rôle.

The real income of the country importing gold must be considered smaller in the new than in the old position provided the old level of gold production fulfilled our condition of equilibrium and there was no deflation and no unemployment in country B.[1] Gold cannot in the present case be regarded as a temporary store of savings, but it has to be bought by commodities which would otherwise have been consumed at home (and/or by forgoing foreign goods other than gold). Any *price inflation* and its consequences resulting from the increased gold influx and/or the net burden of sterilisation is sheer waste, as the work of exchange of goods was accomplished equally well before. Country B will pay for its predilection for gold by (*a*) incurring the interest charges on sterilised gold,[2] (*b*) exchanging either commodities or interest-bearing assets for gold.

IV

THE PRESENT POSITION AND THE PROBLEM OF THE FUTURE

Recent statistical literature provides ample material on the magnitude of the factors discussed above.[1] Only the briefest summary is, therefore, necessary.

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[1] Employment will not be affected as the increase of exports or the substitution of home for foreign goods will offset the first shock of increased rates of interest. In the absence of sterilisation the fall in real income will be caused by a greater depreciation of currency than the increase of internal money prices and incomes.

[2] Its gross national debt will also increase. In so far as gold is not re-exportable and has a very uncertain value except for the fact of the statutory price in country (B), it is questionable whether the net debt should not also be regarded as increased.

1. Gold production increased from about £80 millions per annum in 1929 to about £245 millions in 1936. A further increase to a maximum of between £280 and £350 millions is likely if basic conditions do not change. The increase in Russian production included in these figures is open to doubt; it has as yet only partially affected other countries, the larger part having been used to increase internal reserves. The non-recurring additions to supply from India and China as well as Western hoards have already been absorbed. Some countries (Japan, France), however, are still releasing gold.

The elasticity of the supply of gold is very low *in the short run.* Russian output is not influenced by cost-price relationships. The general existence of higher grade ore in South Africa than that now milled prevents any quick reaction of her production to either a fall in price or a rise in costs.

2. Current gold production is being taken up primarily by the United States, which alone among the great powers has a fixed statutory price for gold. The British authorities are not bound to maintain a fixed price for gold. They have at times bought too little to prevent sterling from rising in terms of the dollar and other gold currencies. Most of the weaker countries, even when they are able to buy gold, prefer to hold sterling and dollar securities.

3. The current national income of America is estimated at $60,000 millions (in 1936). The volume of annual savings was put at $17,500 millions before the crisis. It can now hardly exceed $10,000 millions. The current Federal deficit is still expected to reach $2,500 millions. At full employment, but without a considerable rise in prices and incomes, the national income may rise to $72,000 millions, and savings to $12–14,000 millions per annum. The balance of payments of the United States shows a small but growing unfavourable balance on current items including silver, but excluding gold. The import of gold was balanced by a net import of capital into America.

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[1] C. O. Hardy, *Is there enough gold?* Washington, 1936. *B.I.S. Reports VI and VII*, Basle, 1936 and 1937. Colin Clark: *National Income and Outlay*, London, 1937. *Recovery Problem in U.S.*, The Brookings Institution, Washington, 1937, and the bibliography quoted there, especially the publication of the Gold Delegation of the Financial Committee of the League of Nations.

The British gross national income is estimated at over £5,200 millions. Current savings are between £500 and £550 millions. The international payments position is similar to that of America.

4. The fluctuation in the national income of America was of the order of magnitude of $30–40,000 millions. The gross national income of Britain declined by about £550 millions during the great depression. Its subsequent rise is estimated at £1,000 millions.

These facts and the preceding theoretical considerations suggest that the rise in gold production undoubtedly presents a serious problem for a policy calculated to ensure the maintenance of economic stability. The problem must be solved by common agreement and conscious effort.

The possibilities are manifold.

(1) Unchanged price; unchanged freedom of production; no *de jure* stabilisation.
  (*a*) Unrestricted use of gold for multiple credit expansion by England.
  (*b*) Complete sterilisation of the gold production by U.S. and England.
(2) Reduction of the price; maintenance of the freedom of the production,
  (*a*) by introducing an import tax or increasing the handling or seignorage charge.
  (*b*) change in the statutory or *de facto* buying price of the Governments.
(3) Maintenance of present price; artificial limitation of production either by agreed special taxation or by direct limitation, if partial sterilisation and co-operative measures to reduce the impact effect of the gold problem by extending the use of gold should not prove sufficient.

(1*a*.) The increase in gold production was negligible in comparison with the fluctuations in investment reflected in the variation of the national income of the United States or England.[1] In the period of depression current gold production and stocks released by the East served to alleviate the impetus of liquidation, and to produce the monetary conditions necessary for a reversal of the deflationary spiral.

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[1] In smaller countries the increase in gold output must have had an appreciable direct effect on demand. Its tonic influence on business sentiment was of course important even from the point of view of the world.

With the return of confidence and a rise of the expected return on capital investment any additions to the gold stock would be fully utilised for multiple credit expansion. Even if both countries co-operate, this means an injection of additional purchasing power of an amount of the same order of magnitude as the current flow of savings, i.e., about 40–60 per cent. The resulting inflation would be devastating. The low elasticity of supply precludes any early relief. *A fortiori* an attempt to put the whole burden on the shoulders of the United States could not succeed. The external value of the dollar would deteriorate, and long before gold production is affected the policy would have to be reversed.

(1*b*.) A complete sterilisation of the current increment to gold stocks[1] would have to utilise up to about 10 per cent. of the current savings of the two countries. An attempt to continue complete sterilisation therefore is likely to result in a very sharp rise of first the short, and then the longer term rates of interest. Considering the psychological repercussions of such a development, it is safe to conclude that the Governments will not dare to carry it through. Attempts to increase the demand for liquidity which depress expectations directly by frightening the investor and entrepreneur—such as we witnessed between February and June of this year—are very dangerous. They either succeed, and then there is grave danger of the cumulative process reversing itself, or they lose their force and can no longer be utilised.

The consequences of the recent doubts concerning the maintenance of the present value of gold in terms of the leading currencies have demonstrated that vague utterances couched in terms even more cautious than normal Government declarations on policy will not allay the psychological malaise.[2] The probability that the gold scare will at some moment or other recommence is not negligible. Its danger could only be finally eliminated by a very much more definite reassurance about stabilisation than either the Tripartite Agreement or the temporarily salutary statement of Sir John Simon. At the moment, moreover, the belief, however mistaken, that there is a direct relationship between the price of gold and the general price level constitutes a very real risk, and uncertainty about the future of gold is alone sufficient to form the potential starting point for a deflationary[1] process. It was only the strength of the expansive impetus of the present recovery, buttressed by rearmament, that prevented it from becoming dominant in the last few months. Recovery, however, has outlived its first vigour and the scare may re-appear unexpectedly at a much more awkward moment.

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[1] The past increase in gold stock especially as a result of revaluation is either already sterilised or never became effective. This very much diminishes the force of argument in favour of a reduction of gold price.

[2] The bland abnegation of the problem, too, will only arouse suspicion but not allay fear.

It is not to be expected that smaller countries will be willing to take the risk of increasing their gold holdings even if they are in a position to do so, prior to a definite assurance that they will not incur losses thereby. Without their co-operation, however, the present policy of sterilisation will, as has been shown in the preceding section, become a very real and politically dangerous burden in the not too distant future.

(2*a*, *b*.) The attempt to maintain the present uneasy *de facto* stabilisation on, and sterilisation of, gold, without definite and conscious co-operative action to solve the problem, will, in all probability, create a position in which a drastic reduction in the price of gold, either unilaterally or bilaterally, will appear as the only possible solution—if for no other reason because the sheer lack of time will prevent the elaboration of a less abrupt but more complicated method.

Some eminent experts[2] seem to believe that the scarcity of gold, the basis of its monetary use, could be effectively safeguarded by this method. The road which we have traversed in the last ten years shows that alterations in relative exchange rates, however effective in minimising the internal difficulties in the path of readjustment they may appear theoretically, are likely not to cure the old, but to create new and even more acute disequilibria. Considering the position of certain not unimportant countries—e.g., France—and the effect of a reduction of the price of gold on their financial position, the conclusion that new disequilibria would be created is certainly justified. Nor does this reduction in the value of existing stock help the solution of the problem of current gold production unless the resulting loss is covered by taxation or deflation. That solution is politically hardly conceivable.

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[1] The loss of capital invested would have direct and considerable deflationary result quite apart from the more general and psychological influence.

[2] Professor Cassel, contrary to his earlier opinion, now thinks this method the only satisfactory one (*Daily Mail*, June, 1937). Mr. Brand seems implicitly to lean towards this final solution.

A drastic reduction of the price of gold by unilateral action or without instantaneous *de jure* stabilisation would, moreover, tend, not to cure, but to aggravate the problem of abundance concentrated in the strongest countries. Existing stocks are incomparably greater than current output. A fall in the value of gold without *de jure* stabilisation would certainly extinguish the last vestige of confidence in the present monetary arrangements. There would be acute uncertainty as to whether further reductions should prove to be necessary. The volume of purchasing power required in terms of revalued national currencies to take over the output of gold would fall in proportion with the price and the problem of sterilisation be mitigated. The volume of output would, however, probably not decrease and might increase, at least in the short run. This would further intensify fears about the future course of gold policy and consequently the weaker countries would try to sell their stocks. This dishoarding process would undoubtedly force further reductions in the price of the metal and perhaps lead to the final collapse of the gold standard. The events of the last few months and the history of silver in the latter half of the last century provide a useful if deterrent example. No partisan of this method, however, has urged the desirability of demonetisation. The capital losses and the crisis of confidence, moreover, which would certainly ensue might well engulf the economic system in a new deflationary spiral. The " mystic " quality of gold can be maintained only so long as its value in general appreciates.[1] Once the trend is reversed confidence vanishes. A *de jure* stabilisation might preserve confidence. But a *de jure* stabilisation immediately after a momentous change whose consequences are unknown is at least improbable.

(3) A far more flexible and less violent expedient seems desirable. The increase of the liquidity of the banking systems under the present disturbed conditions, which easily lead to capital movements, seems to be a reasonable precaution. The declared maintenance of about the present price, partial sterilisation not causing a violent rise in the rate of interest, measures to expand the demand for gold, co-operation in monetary policy and, if these steps do not suffice, a control

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[1] The " return " of individual currencies to gold (i.e. to or towards their previous gold content) after revolutionary and war inflation never had the psychological significance of a reduction on the value of gold.

of production by whatever methods seem opportune, appear on the basis of this investigation the most rational—though not the most probable—policy.

The root of the resistance to a more or less definite stabilisation is the strong though rather recent belief that the dilemma of internal versus international stability, of stability of prices or business activity versus stability of exchanges, is insoluble. The existence of large sterilised gold reserves, the greater harmony between the social policies of, at any rate the democratic, countries, the refinement of methods to prevent dumping, are new factors which rob that belief of most if not all of its real basis. Stabilisation could be made flexible,[1] though the history of the last few years has made people less afraid of legislative action and its consequences. If a rational monetary policy[2] is followed on the basis of an initial settlement which does not crystallize disequilibria between countries but provides by common agreement a stable base for all, the feared dilemma need not and should not arise.

A merely partial sterilisation of the increment in monetary gold stocks would, as long as present optimism prevails, encourage some inflation. Credit structures might perhaps be allowed to expand just enough to permit the absorption of the gold by the banking system without the sale or reduction of other assets. It is difficult to predict exactly what consequences such a policy would have on prices and production, as the velocity of circulation might rise violently at a later stage of the boom and distort the picture. In a world where risks are high, national debts and social costs are increasing, and the population is tending to fall, one is inclined to prefer this undoubtedly existing risk to that of continued stagnation as a result of the refusal of entrepreneurs to invest. A slow but steady increase in prices would go a long way on an easier path to prevent the deadlock predicted by Mr. Keynes arising out of the impossibility of depressing the rate of interest low enough to offer an incentive to investment. This policy would, through an increase of imports, promote the ability, and the restoration of confidence in the future of gold and a gentle but firm moral suasion would increase the willingness, of the smaller, and especially of overseas, countries, to purchase gold. A conversion of existing foreign exchange reserves into gold would sterilise much more than a year's output. The restoration of confidence in France would provide a further considerable relief. The strain on the *current* savings of the two Great Powers would be thereby eased.[1] In view of the phase of the business cycle at which these Powers find themselves at the present moment, it is essential that such relief should be afforded as soon as possible. It was easy enough for either or both of them to absorb gold during a depression, in the quantities now forthcoming, with or without sterilisation. The problem only arises in or on the verge of a boom. The debtor countries should be given help and incentives to work out their own salvation, both by fair settlement of the old debts and a new liberal commercial policy. Productive direct investment ought not to be restricted by authoritarian bodies established in the period of failing monetary strength. The advocacy of foreign loans to solve the gold problem, however, disregards the clearest warning of the last so-called reconstruction period. Prior to the solution of the problem of economic and monetary policy the loans would not " absorb " gold but be recklessly spent on armaments or used to repay old debts. If a repetition of the general inflation is to be avoided promiscuous lending must not be again encouraged.[2]

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[1] Tactically a definite *de facto* stabilisation is perhaps preferable, as it permits bringing pressure on other countries. Some readjustment of the price of gold need not be ruled. It is probable that the consequences of a say 10 per cent. reduction are already discounted and need therefore cause no deflationary repercussions. In the case of small countries, depending on sensitive export trades, these arguments do not apply. They should maintain freedom to change their rates of exchange in case of need.

[2] On the basis, e.g., of the enlightened memorandum of the B.I.S. to the World Economic Conference, 1933. Cf. B.I.S., 5th report, covering 1934/35, Basle, 1935, pp. 41–48.

It may well be that after a time the authorities will come to the conclusion that the *status quo* cannot be maintained, either because of the force of the inflation thereby engendered, or because the strain imposed by the attempt to prevent it, proves too strong. In that case a restriction of the production of gold is probably the expedient calculated to minimise inevitable disturbances. It does not affect the value of the existing stock and therefore does not lead to a cumulative process of selling. It provides an instantaneous drop in production which is difficult to obtain in the case of Russia and South Africa by a reduction in the price of gold. It is flexible and reversible. An increased demand for commodities by Russia, and a direct stimulation of consumption and investment by the gold producers of other countries may yet be welcome. The world has learned in the last few years how to regulate the output of commodities much less easily controlled than the bulk of gold production.[1] There seems to be no reason why such regulation should not be extended to the gold industry. It would cause far less disturbance than an indiscriminate slashing of the price with all its untoward repercussions on established capital values. Gold producers would, certainly, suffer some loss. This loss could be more equitably distributed than by other methods. And above all the indirect and much more important general scare of deflation would not be raised. In the case of gold, whose special position arises out of its guaranteed price, a direct regulation of production is logical. *Laisser faire*, on the production side only, can hardly be called a rational arrangement. The re-establishment of flexibility on the demand side, however, would be harmful in its repercussions and impossible in practice.

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[1] Especially an agreement with the Russian Government providing for a concentration of its war chest held abroad at the Central Banks would be important and feasible.

[2] The flotation of a foreign loan to re-establish gold reserves is probably the most unsound procedure conceivable. It encourages too high stabilisation rates, burdens the country without helping to increase the capacity to repay. The League Loan's sorry story should provide a warning.

The operation of the system advocated is not simple. But it gives us a rational expedient to regulate our monetary destiny. To demand safety, certainty and simplicity in the present world is to court failure and disaster.[2]

[1] Most gold mining countries and also the non-industrial sources are of small importance. There may be some "bootlegging". There is also illicit diamond buying. Regulation is nevertheless effective.

[2] It would be politically advantageous and calculated to re-assure America if the remainder of the fiduciary issue was to be replaced by gold. A further measure permitting the absorption of gold—at reasonable cost—would be the increase of the reserve ratio of the Joint Stock Banks to 15 per cent. or 20 per cent. If their deposits are allowed to rise *pari passu* their earning power will not be affected. These two measures would permit the total or partial sterilisation of £400–450 million sterling.

Uncertainty and Liquidity-Preference

By L. M. Lachmann

I

The prominent place which the concept of liquidity-preference has been occupying in recent literature on economics may be regarded as an indication of the urgent desire for an exhaustive investigation into the forces determining the demand for Money. The importance of the problem for both theoretical and practical purposes need not be stressed. In monetary theory no satisfactory progress can be hoped for without the help of marginal utility-analysis, a type of investigation which is inapplicable as long as we remain ignorant of the character of the need which the possession of Money satisfies. For monetary policy, on the other hand, it is vital to have a satisfactory criterion for the division of the total quantity of Money into the " effective " (business-funds) and the " ineffective " (hoards) circulation, if we want to be able to form reliable estimates of the efficacy of monetary measures.

In analysing the forces which determine the demand for cash-balances the economic theorist cannot hope to accomplish his task adequately, if he confines himself to the enumeration and classification of the various " reasons for holding Money ". In this field, as we shall see presently, " motives " are, indeed, a tricky subject.

For in examining the causes of liquidity-preference we are actually side-stepping from the highroad of economic science, where preferences are identical with motives and must not be further analysed. Whereas, in general, economic theory strives to reduce all economic phenomena to consumers' (and savers') preferences, in the case of Money this procedure will not do. Why the connoisseurs of modern painting prefer pictures by Picasso to pictures by Utrillo is a question with which it has been generally agreed that the economist is not concerned. But why sometimes owners of wealth prefer holding their assets in form of cash-balances to other forms of investment is a problem which the monetary theorist cannot afford to shirk. The causes of liquidity-preference have therefore to be sought in a field which lies beyond the realm of preferences: in our case preferences cannot be regarded as ultimate determinants.

But where have these causes to be sought, and by what forces are they determined? In order to overcome this dilemma we have to remember that what as social scientists we are concerned with are not individual acts but mass-phenomena. Mass-phenomena have to be made intelligible by reference to the similarity of the conditions under which different individuals have to act. The conditions the similarity of which makes different individuals, who are subject to them, act in an identical manner, may be either of a subjective (psychological) or an objective (institutional) nature: i.e., they may either exist purely in the imagination of the acting individuals or have a (socially) objective existence. Thus, e.g., the financial panic often accompanying the outbreak of a major crisis may either be explained by taking recourse to alleged phenomena of mass-psychology (the famous "waves of optimism and pessimism") or by the working of certain institutions like banks.

Men may act identically, either because they are all subject to the same mass-psychological influences or because they all have to operate within the same institutional framework. As our knowledge of mass-psychology is rather scanty compared with our comprehensive cognition of institutions and the way they work, it might be useful to lay down as a preliminary rule that if in the course of the following investigation a mass-psychological and an institutional hypothesis come to compete for the rôle of a "cause", preference will be given to the latter.

II

In recent literature there seems to exist a fair amount of agreement among writers that Uncertainty has to be regarded as the major determinant of movements in the size of cash-balances, i.e., as the main cause of liquidity-preference. At closer inspection, however, this apparent harmony emerges as somewhat problematical, because different writers give this word a different meaning. In the following we shall confine ourselves to the examination of two examples of monetary theories in both of which the leading rôle is assigned to Uncertainty, and we shall find that in each case the word has a different meaning. Afterwards we shall make use of the result of our critical examination of these theories in order to find that meaning of " Uncertainty " which will enable us to regard it as the cause of liquidity-preference.

In a study on " The Co-ordination of the General Theories of Money and Price"[1] Dr. Rosenstein-Rodan uses the word Uncertainty " in its original meaning of doubt, of a vague and dumb feeling of not knowing ".[2] This " vague and dumb feeling " is, however, according to him, of causal significance for the existence and magnitude of cash-balances. This result is obtained by means of contrasting our world of frictions and unforeseen changes to a frictionless world of perfect foresight. " In an economy without frictions, where everybody foresaw with perfect certainty his tastes, income, future prices, and therefore the dates as well as the size of his purchases, nobody would keep a cash-balance " (271). Hence, " Money (as cash-balances) exists only and in so far as general foresight is not certain, it is the function of the individual's feeling of uncertainty, a means of meeting it, a good satisfying the want for certainty " (272).

This statement, however, is later on qualified in a footnote, where we read that " The function of uncertainty about income and prices will be *naturally*[3] a function of many variables " (p. 272, footnote 3); i.e., translated from the language of functional into that of causal-genetic analysis, Uncertainty is but one of many causes of liquidity-preference. Moreover, the statement about the functional relationship between Uncertainty and cash-balances has to be read in the context of Dr. Rosenstein's discussion, in an earlier part of his article, of the store-of-value-function of Money (266/67). Here we are told that Money, although being the only good capable of fulfilling the functions of unit of account and medium of exchange, is " only one of many goods fulfilling what one may call a cash-balance-function ". And, significantly enough, the author adds that, even where Money is established as the standard of deferred payment, it will not be the only store of value.

The conclusion which we have to draw from Dr. Rosenstein's argument thus appears to be this: There exists a certain functional relationship between Uncertainty and the demand for cash-balances. Yet, neither of the exact form of this relationship nor of the other variables and their relative significance have we the slightest knowledge. We are left much in the position of King Crœsus, who asked the Oracle of Delphi about his chances in the war he was going to wage on his neighbour and received the reply that he would destroy a great empire.

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[1] ECONOMICA, August 1936.

[2] *Op. cit.*, p. 272, footnote 2.

[3] Our italics.

If a causal-genetic investigation like ours is to be conducted on Dr. Rosenstein's line of approach, two main questions have to be answered. Firstly: Can increased Uncertainty not be accompanied by a diminution in the demand for Money, viz. a "flight from the currency"? Secondly: Are we not familiar with examples of cash-balances held not because of Uncertainty but because of the (subjective) certainty of their owners that prices will fall? If in both cases the answer is in the affirmative this would mean that it is impossible to assign to Uncertainty *in this sense* the rôle of a cause of liquidity-preference.

In both respects the lesson of experience seems to be clear. Uncertainty may result in an increased demand for cash-balances. But if at the same time the future of the currency is deemed to be in danger, we know by manifold experience that people will feel induced to exchange their cash for highly illiquid goods like furs, jewels, and furniture.[1] Uncertainty may generally induce people to restrict the volume of their current liabilities. But if it also affects the future value of Money, people may even borrow for buying commodities of the kind mentioned. When will they act in this, when in that manner? We do not know; our functional relationship does not provide us with a satisfactory criterion.

While our conclusion is thus of little, if any, help to the monetary theorist and, of course, entirely useless for purposes of monetary policy, it would be unfair to blame Dr. Rosenstein for it. For the task he had set himself was to co-ordinate the general theories of Money and of Price, and since he endeavours to do it on the Lausanne model, he is entitled to confine himself to inserting a new set of variables into a system of simultaneous equations. As in this system all quantities are interdependent, it is sufficient to show that any variation in Uncertainty must have some repercussion on some other magnitudes in order to make the system work and safeguard it, on its own level of abstraction, against theoretical objections. That this type of functional analysis does not solve the problems we have set out to solve is no argument against it; that it " does not tell us anything about the real World ", is a true, but by no means a novel gravamen. Slightly varying Professor Ricci's comment on Pareto one feels tempted to say that, while adding with great architectural skill to the beauty of the famous castle that was erected on the shores of the Lake of Geneva, Dr. Rosenstein has still left the housing problem unsolved.

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[1] It might be objected that the concept of liquidity-preference can be given a wider meaning by divorcing it from the demand for Money and extending it to all commodities. People may thus wish to be liquid in furs and jewels as well as in Money.

It seems, however, that, if thus far extended, the concept would lose all concrete meaning and liquidity-preference become indistinguishable from general preference. If every single exchange transaction can be explained by liquidity-preference on both sides, how are we to say who has become more liquid and who less?

One might even go a step further and argue that in such cases different commodities become Money for different people. This would, of course, mean the end of all monetary theory.

III

Perhaps our failure to establish a causal relationship between Uncertainty and liquidity-preference is due to our having used the word Uncertainty in too wide and too imprecise a meaning. Perhaps it is futile to believe that any definite form of conduct can be the outcome of " a vague and dumb feeling of not knowing ". On the other hand, our failure to derive a definite demand-function from Uncertainty in its most abstract and general sense suggests that a better result may be expected, provided the notion of Uncertainty can be given a more concrete meaning. Perhaps, if we can make our notion more concrete by providing it with an " object " of Uncertainty, by making clear what exactly people are uncertain about, our endeavour will be attended by more success.

For Mr. Keynes in his " General Theory of Employment, Interest and Money " Uncertainty in such a concrete sense, i.e., a very special kind of Uncertainty, is, indeed, the main cause of fluctuations in liquidity-preference. It is Uncertainty as to the future rate of interest which, according to him, " is the sole intelligible explanation of the type of liquidity-preference " which he believes to be of importance (201). Among the motives for liquidity-preference we first come across the *transactions-motive* (which later on is subdivided into the income- and the business-motive, p. 195), described as "the need of cash for the current transaction of business and personal exchanges", and thereafter "the precautionary-motive, i.e., the desire for security as to the future cash-equivalent of a certain proportion of total resources" (170). Both these are, however, treated by Mr. Keynes as being of no causal significance and as mere functions of total income (Y).

"There is, however, a necessary condition failing which the existence of a liquidity-preference for money as a means of holding wealth could not exist. This necessary condition is the existence of *uncertainty* as to the future of the rate of interest, i.e., as to the complex of rates of interest for varying maturities which will rule at future dates. For if the rates of interest ruling at all future times could be foreseen with certainty, all future rates of interest could be inferred from the *present* rates of interest for debts of different maturities, which would be adjusted to the knowledge of the future rates" (168). In the absence of such certainty we have thus the third incentive to liquidity-preference in "the speculative-motive, i.e., the object of securing profit from knowing better than the market what the future will bring forth" (170). In other words: If the price of 4 per cent. bonds stands at 100, a person expecting the rate of interest to rise by more than 0·16 per cent. in a year will gain by selling his bonds and waiting for the new price-level.

This seems plausible enough, but two sources of doubt remain. In the first place, why is the motive of an activity purporting to secure profit from speculation described as "Uncertainty"? A man who believes that he knows the future rate of interest better than the market, and who is prepared to forego present income in order to secure future profit from his apparently superior foresight, must, it would seem, be pretty certain of his "hunch". In what sense can we call him "uncertain"? To this Mr. Keynes' reply would probably be that he is uncertain whether the present valuation of the market is the right one. This leads us to our second doubt arising out of Mr. Keynes' peculiar assumptions about the structure of the market for securities.

Mr. Keynes attaches great importance to the existence of an organised market. "For in the absence of an organised market, liquidity-preference due to the precautionary-motive would be greatly increased ; whereas the existence of an organised market gives an opportunity for wide fluctuations in liquidity-preference due to the speculative-motive " (170-71). At closer inspection, however, Mr. Keynes' organised securities market emerges as a particularly hybrid type. For it appears that, " perfect " though his market may be in any meaning attributed to this word by traditional " classical " doctrine, it certainly is *not* organised with a view to facilitate those operations we are most interested in, i.e., intertemporal exchange-transactions actuated by the speculative motive. In other words, it is just because Mr. Keynes' market is not an organised forward-market that here " bearishness " entails liquidity-preference ! For on a market which is organised for intertemporal exchange, everybody is able to express his expectations for the future by buying or selling for delivery in the future.[1] On an organised forward-market it would certainly not be true that " the individual, who believes that future rates of interest will be above the rates assumed by the market has a reason for keeping actual liquid cash, whilst the individual who differs from the market in the other direction will have a motive for borrowing money for short periods in order to purchase debts of longer term " (170). On an organised forward-market both individuals could express their expectations by forward-transactions which do not require any cash. Where the market for securities is fully organised over time, the owner of 4 per cent. bonds who fears a rise in the rate of interest has no incentive to exchange them for cash, for he can always " hedge " by selling them forward.

We do not, of course, mean to deny that, with or without a forward-market, expectations will affect the rate of interest. As there are no carrying-costs involved in keeping securities, spot-price and forward-price must always be equal. Whether securities are sold spot or forward will thus make no difference as far as prices are concerned. What it will make a difference to is liquidity-preference, for whereas spot-transactions involve cash-payments, forward-transactions do not. On an organised forward-market for securities " bearishness " will therefore not lead to a desire of bond-holders to exchange their assets against cash, but to a desire to "hedge", which will bring down the forward-price and, via arbitrage, also the spot-price. Negligible effects on liquidity-preference may here occur to the extent to which the spot-market "lags" behind the forward-market; that is to say, to the extent to which spot-buyers are not quick to lower their demand-prices in accordance with the lower forward-price, and actual spot-transactions are necessary in order to adjust the spot price, which otherwise will almost automatically—and without any actual transactions being necessary[1]—follow the forward-price.

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[1] As is clearly shown in Ch. 17, where " commodity-rates of interest " are treated as intertemporal exchange-rates, Mr. Keynes is, at times, well aware of the necessity of introducing an intertemporal market in order to explain intertemporal exchange-relationships.

IV

Our attempt to establish a causal relationship between Uncertainty and liquidity-preference has so far turned out to be a complete failure. When we interpreted Uncertainty in the widest possible sense, we learned that it might lead to anything and therefore could prove nothing. When we tried to give it a narrower and more concrete meaning, we had to realise that with organised intertemporal exchanges no appreciable effect on liquidity became visible. Confronted with this dilemma we have to fall back on the other "motives for liquidity".

There is, first of all, the precautionary motive. "To provide for contingencies requiring sudden expenditure and for unforeseen opportunities of advantageous purchases, and also to hold an asset of which the value is fixed in terms of money to meet a subsequent liability fixed in terms of money, are further motives for holding cash" (196).

The first part of this sentence is left comfortably vague, since, as we have seen, for the purpose of meeting unforeseen circumstances, Money is just as good or as bad as any other good, and all depends on the nature of the circumstances. Its second part, however, contains the whole truth. If by Uncertainty we understand the anxiety of the debtor, whose debt is due on demand, regarding the future actions of his creditor, then, at last, we can say that Uncertainty is the cause of liquidity-preference.

This may sound quite plausible. The danger, however, is that economists who are brought up in an atmosphere of genuine contempt for "institutionalist" argument will admit the possibility, but refuse to see its implications. And while probably prepared to grant that there might be some connection between the existence of money-debts and the demand for Money, they may not accept our main contention that it is only because of this connection that such a thing like liquidity-preference does at all exist, and that the sole intelligible reason why people prefer Money to commodities is the debt-discharging quality of the former. They may admit that Uncertainty of the type described is *one* of the causes of liquidity-preference, but will probably deny that it is the only one.

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[1] It is another of the cases (cf. Keynes, *op. cit.*, p. 199) where, in the language of the financial press, "the movement of prices is out of all proportion to the volume of dealing."

As it is obviously impossible to demonstrate a contention like ours by a process of logical deduction, we shall have to convince the reader by drawing his attention to the difference between the functions of Money and Money's exclusive function, between what Money *does* and *what only Money can do*.

Money is an economic good, and Money is legal tender. Is there a necessary connection between the two? At first sight apparently not, for Money fulfils several functions. Being an economic good it has utility which is derived from the satisfaction of several wants.

In the first place, as medium of exchange, it yields indirect satisfaction, i.e., its utility is derived from that of the goods it is exchanged against. This has in our case the particularly awkward consequence of making marginal-utility-analysis inapplicable. For where both utilities (that of Money and that of the good it is exchanged against) are derived from the satisfaction yielded by the same good (the non-Money-good) we cannot attribute changes in the exchange-ratio to changes in either of them.[1] As, however, liquidity-preference and marginal utility of Money are two expressions for the same thing, it follows that the relationship sought cannot be established.

In other respects Money yields direct satisfaction,[2] e.g., in its function of a store of value. But we have already seen that in this respect it has almost as many substitutes as there are (non-perishable) goods, and that it is impossible to predict when it will be used as store of value and when something else. Sometimes the satisfaction derived from its possession will be even more "direct". Molière's Harpagon, e.g., derives as much "direct satisfaction" from the contemplation of his hoarded treasures as does the spectator from seeing him on the stage. Yet, Harpagon—although a type rather than a character—is not a type appearing frequently enough in reality to account for the mass-phenomena we observe. Those fluctuations in the demand for cash which we observe in reality and have to explain in theory cannot be reduced to the preference-scale of an Harpagon.

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[1] Cf. Tj. Greidanus: *The Value of Money*, London, 1932, pp. 111–13.

[2] We can leave out here its function of unit of account, because its utility in this respect does not depend on its actual possession.

At this juncture let us remember what at the beginning we said about mass-phenomena and their alternative explanation in terms of mass-psychology or institutions. There are, no doubt, some economic phenomena—mostly connected with the "psychology of markets"—which justify and require an explanation in terms of mass-psychology. But with most of the manifestations of liquidity-preference this is far less obvious, and even if it were, our present knowledge of this kind of psychology is, at any rate, too inadequate to cope with them. Therefore, in order to account for those recurring oscillations in the demand for cash which manifest themselves as identical actions of a multitude of individuals, we have to fall back on the institutional setting within which these individuals operate. Their identical actions have to be explained as the outcome of identical objective conditions in which they find themselves.

Of all the institutions within the framework of which the human actions described by economic science are performed, the existence of money-debts is doubtless one of the most important. Now, Money is the legal means of payment, i.e., its owner can use it for discharging debts.[1] This is the only use in which it has no substitutes, for its very institutional character excludes that. On the worst of days, when all instruments of exchange fail us, when all markets and banks are closed, when the most liquid assets have become entirely illiquid, Money—and only Money!—will still serve to discharge a debt. But it must be added that this may easily be the only service it would render under such circumstances; whether it would buy even the smallest quantity of food is rather doubtful.

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[1] It might be argued that this assertion is either tautological (if restricted to money-debts) or untrue (if extended to non-money-debts). In reality, however, it is neither the one nor the other.

Firstly, there is an important distinction between money-debts and commodity-debts: Every commodity-debt is a potential money-debt, because the debtor is able to convert his debt into a money-debt by withdrawing from the contract and paying damages. In the case of a money-debt, on the other hand, conversion into a commodity-debt is legally impossible.

Secondly, it is practically impossible to conclude intertemporal loan-contracts in anything but the money-form. All attempts to do it on some other basis have so far proved entirely unsuccessful (witness the deplorable fate of the Gold Clause in the jurisdiction of all countries with depreciated currencies!).

It therefore seems legitimate to infer that (apart from the reasons implied by intertemporal imperfection of markets) it is principally because of its debt-discharging quality that Money is demanded. The fluctuations in the demand for cash which we observe in reality have thus to be explained as manifestations of debtors' Uncertainty. As the security of a loan depends on the financial strength of the debtor, i.e., the relative value of his assets and liabilities, it follows that every time the money-value of assets has declined, creditors will demand repayment of loans, and debtors will have to take precautions. What this entails on Trade Cycle Theory we shall see presently.

V

Before proceeding to the application of the results of our study we still have to examine the "transactions-motive". Two questions arise: to what extent does this motive determine the demand for cash? And: How far is Uncertainty here of causal significance?

While Mr. Keynes on the whole rather tends to minimise the importance of the transactions-motive for the demand for Money, the point has recently been stressed by Mr. D. H. Robertson.[1] He accuses Mr. Keynes of having neglected the difference between Money in the hands of entrepreneurs and Money in the hands of the public ("those who desire to hold more money and those who desire to use it")[2] and emphasises the close connection between the profitability of investment and the magnitude of the business-funds. As the profitability of investment depends on the degree of confidence which enters the "business outlook", the inverse proportionality between Uncertainty and liquidity-preference seems thus rather firmly established.

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[1] D. H. Robertson: "Some notes on Mr. Keynes' General Theory of Employment." *Quarterly Journal of Economics*, November, 1936, pp. 168–91.

[2] *Op. cit.*, pp. 176–77.

It is, however, difficult to see why an increase in business activity should necessitate a greater demand for cash for the business-funds. Most commercial transactions in the strict sense (that is, excluding wage-payments and retail-sales) are not carried out against cash. An entrepreneur who wishes to extend his scale of operations can almost invariably obtain credit from the producer of the intermediate products he needs, who thus substitutes claims on his customers for commodity-stocks. Moreover, during periods of industrial buoyancy it is quite usual that bills and similar quasi-liquid assets come to be regarded as being " almost liquid " and are used like Money. " Trade creates its own means of payment " ; there is therefore no such thing as a necessary relationship between total output and the size of business-funds.

If, nevertheless, in reality we observe that enhanced business activity is usually accompanied by increased demand for Money, the explanation has to be sought in Uncertainty of the type mentioned. Business men generally hold cash in proportion to the liabilities falling due in the nearest future. What therefore affects their liquidity-preference is not the absolute level of output, but the rate of increase in their short-term-liabilities. Hence every (even expected) deterioration of the conditions under which credit may be obtained (or extended) will be reflected in the state of liquidity-preference.

VI

From what we so far have been setting forth there seems to emerge the general conclusion that Uncertainty may be regarded as the cause of liquidity-preference, if the former is referred to the relationship between creditor and debtor. We shall now apply it to Trade Cycle Theory and will attempt to tackle, by its help, a problem which has, of late, attracted the attention of students in this field.

All those who continue to be sceptical regarding the explanatory value of such formidable devices as the " Relation " and the " Multiplier "—or, more precisely, regarding their alleged interrelationship—will have to look out for possible explanations of the fact that economic crises do not ordinarily lead to the readjustment we should expect in theory, but almost invariably degenerate into " cumulative downward-processes ". Once the " Secondary

Depression" has been recognised as a distinct phase of the cycle and a separate problem of Trade-Cycle-Theory,[1] the problem of its causation and necessity poses itself, and it is towards the solution of this problem, that, on the strength of our conclusions, we believe we are able to make a contribution.

In the modern world banks are the most important debtors of loans which are due on demand. On the other hand, the banks are likely to suffer losses already during the "Primary Crisis", because the processes which have to be liquidated at its end have, directly or indirectly, been financed by bank-credit. In this situation the obvious way out seems to be the immediate writing off of all losses and a subsequent reconstruction of the capital of the banks concerned. There is, however, an alternative solution: As the security of bank deposits is now impaired by the losses the bank has sustained, the latter may now choose to compensate its depositors for diminished safety by increased liquidity. As the banks' Uncertainty in the above-mentioned sense must naturally increase, they may try to appease the fears of their creditors by showing them that they are prepared for all possibilities. But this will, of course, only be necessary if the capital of the banks is not immediately reconstructed; for, in the latter case, the security of the deposits will at once be restored.

Now, there is one good reason why we should expect the banks to go in for increased liquidity rather than for immediate reconstruction. For reasons into which we need not go here the rate of interest is likely to be very high during the period under consideration. The new capital which has to be borrowed for the purpose of reconstruction will therefore be relatively expensive, and it is quite natural that the owners of the banks should try to postpone this operation to a point of time when new capital will be cheaper. But as, for the reasons mentioned, depositors must be compensated somehow, the banks will in the meantime have to raise the cash-proportion of their assets. Contrary to Mr. Keynes' theory we find that it is the expectation of a future *lower* rate of interest which, because of the increased Uncertainty of the banks, leads to enhanced liquidity-preference.

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[1] On the concept of Secondary Depression cf. W. Röpke: *Crises and Cycles*, London, 1936; and G. Haberler: "Some Reflections on the present situation of Business Cycle Theory," *Review of Economic Statistics*, February, 1936, p. 7.

Secondary Depression would thus have to be explained on lines exactly similar to the theory of the boom. By raising the rate of interest above the marginal efficiency of capital the banks appear to cause Secondary Depression and to impose on the economic system a cumulative deflationary process the further stages of which need a more detailed investigation on the lines just sketched out.

As regards practical policy the conclusions of our investigation seem to go far to vindicate the maxims of " orthodox " banking. For in the light of our results the idea of a cheap Money policy as a panacea for crises and depressions loses most of its attractions. And a rigorous banking policy which compels the banks to undertake their immediate reconstruction after the outbreak of the crisis (and which, incidentally, would enforce the early closing of all those banks where this is no more possible) would appear to be the most appropriate method of averting the horrors of the cumulative process of depression.

A Note on Time Elasticity of Demand

By Henry Smith

The writer is indebted to the officials of the London Press Exchange for details of several cases in which the result of a reduction in price of a branded and advertised commodity has been an absolute reduction in sales, which has persisted for some time. It is also common knowledge that the full elasticity of demand for most commodities is only established after a period of time, which may be of considerable length, has elapsed after the change in price. These two phenomena are not unconnected.

The following pages embody an attempt to formulate the conditions governing "time elasticity" of demand, and conclude with a suggestion for its measurement. The argument is throughout confined to downward price movements, but the results seem applicable, with obvious readjustments, to upward movements.

If the price of X falls from P to P′, demand will increase if, (*a*) the demand for X is elastic, and if (*b*) X at price P′ is recognised to be the same good as X at price P. X at P′ is not an identical bundle of utilities to X at P: No good of which the quantity Q is bought is a similar bundle of utilities to those embodied in the same *commodity* when a different quantity, Q′, is bought. If more is bought at P′, it is because Q′ units of the new bundle of utilities has a marginal utility equal to the marginal utility of the final unit of income spent upon the same commodity when Q is purchased at price P[1]. If demand curves are to be drawn, we must assume that the demand, Q, Q′ . . . for X at P, P′ . . . is for these successive bundles of utilities. Thus case (*b*) is only concerned with the physical identity of the commodity at the old and new prices.

A negative element in the bundle of utilities 'X at P′' is the cost of ascertaining if X at P is identical with X at P′. Thus cost at P′ (P′ being any price except P) is P′ plus this cost: the demand curve for X may be drawn, as in diagram I, 11′ being the demand curve for X if uncertainty concerning the homogeneity of X at different prices is not present, 22′ representing the case where it is present. Let us assume that any change in price gives rise to this uncertainty. This will in time disappear, and if X is still physically the same and tastes remain unaltered, the longer the period of time under consideration the closer 22′ must be drawn below 11′: if a sufficiently long period be taken they will be identical.[1]

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[1] Assuming that the marginal utility of money to the consumer remains unaltered.

Elasticity of demand thus depends on the period under consideration: if the period in which 22′ becomes identical with 11′ be taken elasticity is at a maximum. It is at a minimum if the shortest possible period be taken. It is possible to speak of the elasticity of demand per month, per year, for X, being the proportional change in demand resulting from a proportional change in price at the end of a month or a year, from the change in price. Or we may speak of the time elasticity of demand for a commodity, drawing our demand curve on the normal assumptions, by dividing the elasticity of that curve by the number of days it takes any change in price to call forth the change in demand denoted by the elasticity of the demand curve. If elasticity is equal to unity and is immediately established, time elasticity is equal to unity: if it is only established after a month, time elasticity may be said to be $\frac{1}{4}$ if we take a week as the minimum period in which any appreciable change can be recorded.

This terminology however has the drawback that it appears to assume that the movement towards final elasticity through time is uniform, for which there is no basis, and sounds as if the expansion of demand is going on for ever. Thus it appears that we must speak of elasticity being equal to unity in one month, to $\frac{31}{35}$ in five weeks, and so on: the actual period of time must be empirically established in each case for the concept to be of practical value. This implies dropping the assumption that tastes are unaltered; in the real world one cannot distinguish between a movement from Q′ at P′ towards the point horizontally opposite to P′ on 11′, and a change in tastes. One never knows where the true demand curve lies, and can only be guided by the extent to which demand expands at a new price over what seems to be a reasonable time, as for example, AB in diagram II, where the sales per week or day are measured along OY and time along OX, A being the point in time at which price changes and B being the point at which sales appear to have become stabilised.

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[1] I am indebted to Dr. Neuman for the reminder that 2–2′ need not of necessity lie below 1–1′; it may lie above it, or it may intersect it, as in those cases in which over some ranges of price a change in price will "unduly" stimulate demand in the very short period, and over other ranges "unduly" depress it. And, of course, the relation between the two curves would be different for upward and downward price movements. It seems, however, probable that their relation in the text is the one which would be most frequently encountered in the real world.

2. It is possible, however, to submit the factors underlying the time elasticity of demand to theoretical analysis. Let us revert to the assumption that tastes are unchanged and the conditions of supply of alternative goods are unchanged. Final elasticity of demand will be established, following any price change, when uncertainty has disappeared. To estimate the period in which this will take place, we need to know why this uncertainty exists for longer than the normal period of consumption of an average purchase of X—why, when the first of the newly purchased quantity has been sampled and its identity established, full elasticity of demand is not immediately established also.

If *everybody* who previously purchased X still purchases some X at P′ even if, for the first time after the fall in price, they buy as an experiment less than they bought at price P, full elasticity would be re-established immediately the new purchase had been sampled. In this limiting case final elasticity would be established after a period of time in which all stocks of X purchased at P and in hand when the price fell to P′ had been consumed, and the first unit of X purchased at P′ had been sampled. When the last in time of these conditions had been satisfied full elasticity would be established. If, however, some previous consumers of X discontinued their purchases when the price fell this will not take place. Is it possible to fit into our framework of assumptions, which are that the physical characteristics of X are unchanged, that tastes are unchanged and that the conditions of supply of other goods are unchanged, conditions which will allow this to take place and will also ensure, as they must, that the phenomenon will be a temporary one? If it is, then the period of adjustment may be lengthened from our limiting case.

If tastes are unchanged, the reasons for which the purchase of X was discontinued will continue, as they depend upon the preference for substitutes at existing prices over X at P′ plus uncertainty, unless the degree of uncertainty can be changed. But the degree of uncertainty can change by the verdict of those who have made the experiment becoming known. If, as soon as anyone has made the experiment, everybody were to be informed (perfect knowledge in Knight's sense) there would be no difference between this and the first limiting case. Indeed the period of the first limiting case, based upon the result of the last in time of the series of experimental purchases, was based upon an assumption of the non-communication of results, and if knowledge be assumed to be perfect, it must be contracted to the time taken by the completion of the first experimental purchase. So, if knowledge is perfect, the period of adjustment may tend to be very short, even if only one consumer prefers the risk of purchasing X at P′ to purchasing an alternative at its established price. If, however, certainty can only be founded upon personal experience (the opposite extreme within our first limiting case) then those who decide in the first place that the uncertainty surrounding X is so great as to outweigh its fall in price will permanently cease to purchase it. But they have been allowed for in the assumption of unchanged tastes upon which the demand curve was originally drawn. Thus there only remain the two versions of our original limiting case, the one with a longer and the other with a shorter period of adjustment according to whether perfect knowledge is absent or present.[1] The demand curve for X will be drawn differently upon these two assumptions : in the former case it will be less elastic than in the latter.

Both perfect knowledge and complete incommunicatability are extremely abstract conditions : in the real world some consumers will make experimental purchases, some will not, some of the former will communicate their opinions to the latter, and some of these will again become consumers of X. Even if unaltered tastes, the identity of X and unaltered conditions of supply for alternative goods be given, in these conditions a demand curve cannot be drawn. The length of the period of adjustment becomes a function of the speed with which experimental purchases are made, with which these results become known to others, and with which the latter respond. Also the elasticity of the demand curve is in this case a function of the number of experimental purchases (which is capable of theoretical ascertainment) and the number of others to whom the results are communicated (which is not). Thus both final elasticity and the time in which it will be attained depend upon the same variable, the problem thus becoming theoretically indeterminate. Consequently one is driven back upon empirical investigation. All that can be done is to allow for changes in tastes as far as possible and to observe the distance on the historical diagram II between A and B for the period of adjustment, comparing the quantity sold at the two prices at A and at B in order to ascertain the elasticity. There is one further possibility which is dealt with in section 4.

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[1] Perfect knowledge may be postulated without necessarily including absolute certainty as to the homogeneity of the product. Admittedly there is no reason to suppose that the product is unaltered through time because the price is unchanged (as in the celebrated case of Tono-Bungay) but it seems reasonable to suppose that changes in price may arouse justifiable suspicions, and to assume that perfect knowledge is not universal omniscience, but results from the existence of a trade paper or of some body like the U.S. Retail Standards Association.

3. It is possible, combining our original assumptions with the further assumption of perfect knowledge, to determine in theory the extent to which demand will fall off immediately upon the changes in price. This will not give the length of the period of adjustment, as under our conditions this is determined arbitrarily by the time elapsing before the first experimental purchase is made: it will determine, given the latter, the equilibrium path by which the demand curve proper will be rejoined.

It remains to prove that in the preceding special cases consumers have not been behaving irrationally in becoming attached to the original price—that we are not dealing with a case of the economics of " neuroses and confused thinking ".

There is no difficulty about the matter in the case of " inferior goods ": in this case the fall in the price of the inferior good sets income free for other purposes. Income elasticity of demand for it is negative, as in the case of bread quoted by Marshall[1] who says " Such cases are rare: when they are met with each must be treated on its own merits ". Hicks[2] shares this opinion. He writes of such a case " It is only possible at low levels of income, when a large proportion of expenditure is devoted to this inferior commodity, and when, among the small number of other things consumed, there are none that are at all easily substitutable for the first ". Thus the task, in view of two opinions of such weight, is to ascertain under what circumstances any commodities, which are not " inferior goods," tend to behave like them for a time, in the immediate vicinity of a long established price.

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[1] *Principles.* 6th. Ed., p. 133.

[2] " Reconsideration of the Theory of Value," ECONOMICA, February, 1934, p. 68.

The question of time is the first to investigate in this problem. The common sense answer to the question of why demand may fall off for a time after any change in price, upwards or downwards, is that people form habits of expenditure which take time to change. Is it possible to illustrate the factors which determine the " long period " problem of changing habits ? This leads to the question " What is the economic basis of a habit ? ", the answer to which involves the use of time in another sense.[1]

The rational man, with whose conduct alone the economist is concerned, balances the marginal utility of leisure against that of money income in order to maximise his satisfaction. But his money income is only a means to the complex of purchasable satisfactions with which it provides him : thus a certain amount of leisure must be sacrificed to considering the merits of alternative avenues of expenditure. It is perhaps most rational to regard this process, with Knight,[2] as part of his productive activities, despite the fact that the normal economic subject of to-day has no choice concerning the proportions of his time which he will spend in acquiring income, but is free to vary the amounts of his time, unoccupied by earning, which he devotes to leisure or alternatively to directing the expenditure of his money income.

At the risk of unduly refining the argument a distinction must further be drawn between the time which is spent in enjoying the real income and that which is spent in selecting the most efficient avenues of enjoyment. If the economic subject enjoys shopping the distinction becomes a little hard to make, but it is possible to keep to the line of the argument by realising that, for example, the fisherman who enjoys buying tackle is really only reconciled to the work of comparing prices by the agreeable things which he views and handles while making the choice. He would be happier inspecting the stock without having to make up his mind.[1]

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[1] The following argument is very largely based upon Dr. Rosenstein-Rodan's article " The Rôle of Time in Economic Theory," ECONOMICA, February, 1934, especially section II, " Time as an Economic Good." My debt to him will become apparent to the reader without further acknowledgment, except in so far as I confine myself to the consideration of some only of the wide range of possibilities which he indicates.

[2] *Risk, Uncertainty and Profit*, p. 69 n. " It seems logically more accurate, however, to include in production everything except the actual experience of satisfaction."

If there were only one or two consumers' goods in the market, the leisure occupied by directing the efficient expenditure of money income would be negligible. It is only when the complexity of the modern world is considered that the importance of habit for the maximisation of satisfactions becomes apparent. The plight of the habitless man in a modern economy would be similar to that in which one finds oneself when one lands in a country where price ratios, methods of amusement, meals, mealtimes, and even the hours of sleep are different from those of one's normal environment. Under these circumstances the amount of time one spends in arranging one's expenditure of leisure and money is very considerable, and, considering the range of possibilities, leads to a final adjustment which inevitably depends upon one's first experiments. It is important to acquire good habits, but even more important to acquire some habits. Shaw says that the man with a perfectly open mind must be a blackguard : in the world of economics he must be a fool, or at least a " mauvais sujet économique ".

Thus there is a rational defence for the man who allows his expenditure to be guided, at least in part, by habit, habit being nothing more than a tacit acknowledgment that too much leisure may be sacrificed in deciding exactly what to do with one's money. All expenditure is a spending of time as well as money, all novel expenditure contains an element of speculation (as consumer's surplus on the new commodity may be less than that on the commodity which it replaces) and a corresponding element of investment, because if the risk that the leisure sacrificed in deciding to buy X and the money cost of X may be both lost is to be justified, it must be balanced against not only the immediate increase in enjoyment resulting from one new successful purchase, but against the permanent increase in satisfaction which may come from a successful experiment in the distribution of one's income.

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[1] I am indebted to Mrs. Joan Robinson for the warning that this is a ludicrously male example. Much shopping is carried on by women whose alternative household occupations are more, and not less, onerous. To a certain extent, therefore, pricing, " window shopping." and bargain hunting may be regarded as an employment of leisure and not as a sacrifice of it. This is in line with Dr. Rosenstein-Rodan's combination of the " negative good " bear-hunting with the good " bear," in the special case where hunting is enjoyed for its own sake.

Thus the man who allows habit to guide his expenditure is behaving rationally if he assumes that the loss of leisure and risk of loss involved in altering his expenditure is greater than the discounted value of his possible future gains. This is tacitly implied in the assumption of equilibrium theory that the facts of the market are all known. There is one further point worth noting at this stage: if the members of our economy are in process of adjustment it is somewhat difficult to draw up any mutually determinate demand curves because with every experiment in expenditure the economic subject makes, he alters his income elasticity of demand for all goods. Consequently, before we can apply the apparatus of equilibrium theory, we must assume either perfect knowledge or a restricted knowledge hedged about by rational estimates of risk.

The forms which risk of loss in changes of expenditure may take are numerous. Foremost among them is the difficulty of comparing the qualities of alternative goods to those now consumed, without investing much leisure in investigation or some money in experimental purchase. This difficulty becomes greater if the outward appearance and the names of the goods are similar and still greater if advertisement is confined to the reiteration of the phrase "X brand is the best". It is probable that in some cases the most rational type of habit is to adhere to one price for a particular class of goods, rather than to one brand.

Let it be assumed that during a period of stable prices such a general adjustment has been made. Consumers have invested time and money in experiment and have adjusted their income so as to derive the maximum satisfaction from their income of money and leisure. They allow rationally grounded habit to guide their expenditure, drawing in leisure and a maximised income of satisfaction the interest on their investment-speculation of an earlier date. As Samuel Butler would say they are now under Grace and not under the Law, having achieved an adaptation to their economic environment so perfect that it has ceased to be conscious.

The price of X now falls to P′: risk and uncertainty re-enter the calculations of each consumer. Let it be assumed for the sake of simplicity that there is only one effective substitute, Y, for X, and that no consumers of X are also consumers of Y. Let it be also assumed that in addition to the elasticity of substitution between X and Y together on the one hand, and all other goods on the other hand being equal to zero, income elasticity for X and/or Y is equal to unity. Having thus isolated the problem it can be stated that if for some consumers the decreased satisfaction resulting from transferring from X to Y is less than that of buying X at the new price, when the risk that the consumers' surplus on the new purchase will be less than that on the old is taken into account in both cases, these will transfer to Y. The others will purchase X as before. On our assumption of perfect knowledge as soon as the first of these makes his purchase (which may be before anyone has transferred to Y) all will revert to the purchase of X.

The artificial simplicity with which our assumption of perfect knowledge invests this treatment unfortunately excludes the possibility of sacrificing leisure to obtain information. If, retaining the other assumptions, this be relaxed, we can consider this alternative equation. There exists in the scale of preference of each consumer a series of equations between leisure sacrificed and other utilities gained, governed by the marginal utility of leisure on the one hand and the marginal utility of alternative satisfactions, on the other. In our original position of universal habituation these were merely potential: there was no sacrifice of leisure instrumental to the maximisation of alternative satisfactions. This is no longer the case. If the sacrifice of a certain amount of leisure will eliminate risk in the purchase of either X or Y, if in each case the risk is the same, and their marginal utilities are the same, the consumer will proceed to purchase that commodity for which the risk can be eliminated at the least sacrifice of leisure. It is now no longer the case that immediately the first purchase of X has been made, all consumers of X will proceed to purchase it on a scale sufficient to establish the final elasticity of demand. The more successive purchases that are made, the more easy it will become to eliminate risk: the corresponding sacrifice of leisure will diminish, more consumers will find it to their advantage to sacrifice enough leisure to reassure themselves as to the continued identity of X, and the final elasticity of demand will only be re-established when all those for whom the ever lessening cost of eliminating risk makes it worth while have reverted to the consumption of X.

This position, as we saw previously, is not determinate in the strictest sense, because the cumulative effect of spreading information makes it impossible to decide under what conditions of elasticity to draw the true demand curve. This difficulty can only be eliminated if some given rate at which information is disseminated can be assumed, and if, finally, knowledge can be assumed to become universal.

4. It is highly improbable that the artificial conditions under which our example has been worked out, those of perfect knowledge, unchanged tastes, unaltered product, and the existence of only one effective substitute, will ever occur in the real world. Consequently its only practical value lies in the extent to which it provides a method (or at least a hint) for the solution of concrete problems. If we possess data from which we can estimate, with some chance of success, the elasticity of demand for a commodity, as for example, by the methods of Professor Schultz,[1] and if we know how great the relative difficulty of obtaining information about the article in question and its most immediate substitutes is likely to be, we can hope to estimate with some accuracy what will be the immediate response to a fall in price, allowing for the elasticity of supply of substitutes. The length of the period elapsing before this final elasticity is reached depends upon the rate at which information about the commodity in question is spread. The assumption of our analysis, that all potential consumers of the product had already consumed some of it must now be dropped for most goods, but the argument as stated can still be employed, for risk now attaches to not trying X when the price falls. Only in a completely static economy is complete habituation rational.

How is this rate to be established, except by comparing similar situations in the past, or by statistical investigation after the event ? As in most questions concerning the application of economic theory there is no one solely correct method. It is probable however that questions connected with advertising play a prominent part in deciding the difficulty of making comparisons between alternative goods, which advertising may either increase or decrease. Moreover it is possible to ascertain what the manufacturers

[1] The writer is not necessarily convinced of the validity of these methods; they are mentioned as an example of the type of investigation which would be necessary in order for this suggestion to bear practical fruit.

of the good in question consider the length of the period of adjustment is likely to be, by observing their expenditure upon advertisement. If, for the industry in question, a "productivity function" for advertising at the old price can be produced, giving the relation between expenditure and increased sales, then if it be assumed that this function will be the same at the new price, or if some adjustment is made on grounds of probability, it can be estimated what the entrepreneur in question expected to be the extent of the temporary decline in demand and how long he expected it to last.

Pending the accumulation of data and the elaboration of technique which must necessarily precede any such attempt, there are three relevant problems which are capable of theoretical solution, all being connected with advertising. The first of these is the case in which the effect of advertisement has been to render price the most easily recognisable "specification" of the good under consideration; the result of continued advertisement at one price has been to render the product almost unsaleable at any other. Thus, if only relatively short periods are considered, the firm appears to operate under conditions bearing a superficial resemblance to those of perfect competition; price is quite independent of output over a wide range. This particular case I have examined at length elsewhere.[1]

The second is the problem of maximising profit under the ordinary assumptions of the short period, with the additional assumption of some time-lag in the consumers' response to changes in price. (The first case is, of course, an extreme example of this.) Most of the theoretical points which can be raised in this connection have recently been discussed at length, but it is of interest to note that the fall in the demand curve drawn on the normal assumptions which appears on each side of the established price, if the period for which it is drawn is short enough, may be partly transformed into a rise in the cost curve on each side of the established price if both "demand" and cost curves be drawn incorporating the effect and the cost of advertisement respectively.[2] If this is the case, demonstrating that the employment of advertising lessens the cost of price changes as such to the firm, the result will be to render prices more mobile and the economy in consequence more stable. If, *ceteris paribus*, the "yield" of advertisement in hastening the reactions of the consumer is greater at lower than at higher prices, it will therefore tend to make prices fall more easily than they rise, and vice versa. This is quite independent of the slope of the combined cost curve and the "demand" (or sales) curve, except in the immediate vicinity of the established price, being only concerned with actual movements of price.

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[1] "Discontinuous Demand Curves in Monopolistic Competition," *Quarterly Journal of Economics*, May 1935.

[2] Cf. my article "Advertising Costs and Equilibrium," *Review of Economic Studies*, October, 1934.

The third problem is connected with Mr. Harrod's long period cost curve.[1] If the envelope of a series of short-period cost curves is to be smooth, the series must present an unbroken gradation of possible sources. Under these conditions, which are only likely to be realised in the real world by an industry in which the factors are fluid but not infinitely divisible and in which, consequently, economies are achieved as a result of greater specialisation as output increases, the producer will change the size of the source each time a change in demand induces him to alter his output. If a secular increase in demand for the product of a firm in this position takes place, and if, in order to simplify the problem, providence has so arranged that the long-period cost curve is the envelope not only of the short-period cost curves but also of the successive positions occupied by the demand curve,[2] the producer will change his price once only for each of his possible changes of scale. Thus the firm moves from one position of equilibrium to another, decreasing its prices as demand increases. If there is any time-lag in the response of consumers to price changes, however, the long-period cost curve will not be wholly independent of the conditions of demand; this becomes apparent when the decisions which the entrepreneur has to make are investigated.

It is highly improbable that if he reduces his price demand will immediately expand to its "real" volume at the new price. Therefore he will be confronted with the problem of *when* to alter his price. If he lowers it before the new plant is ready, he will have to operate the old one for a time at a loss, but he will be able to start his new plant at the capacity for which it was designed (which may not be full capacity in the ordinary sense, if the market is imperfect, and cannot be if the long-period cost curve is sloping downwards) as soon as it is complete. If, on the other hand, he waits till the new source is completed, he will have to operate it for a time below this capacity. Therefore he must estimate how long it will be before full elasticity of demand responds to the lowered price, and so time the alteration of the price in relation to the change in the scale of production as to minimise the cost of transition.

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[1] "Doctrines of Imperfect Competition," *Quarterly Journal of Economics*, May, 1934.

[2] This is not an attempt to read this assumption into Mr. Harrod's argument: he states quite clearly the possibility of error on the part of the entrepreneur who is anticipating changes in the demand for his product. But, if demand for the product of the firm is increasing, it is probable that demand for the most immediate substitutes for that product may be increasing, and if this is the case and all similar firms enjoy similar economies of scale, the firm will be forced to reduce prices and commence production on a larger scale. Whether a position of equilibrium be achieved at once, or whether it be the result of a series of oscillations, that position will satisfy Kahn's conditions, as the gradation of possible scales of plant is unbroken. If the entrepreneur be assumed to possess perfect foresight there is thus no difficulty about the task which has been assigned to providence.

This cost of transition he will add to the capital charges of the new plant. Very possibly the effect of an advertising campaign directed to shortening the period in which full elasticity is established will be to lessen this cost. If this is the case it will lessen the average cost of operating the new plant to a corresponding extent. Thus one may take as a limiting case the condition in which there is no delay in the response of demand to a change in price, and consequently no specific cost attached to price changes. In this case the long-period cost curve is the envelope of the short-period cost curves, which may be drawn to include the most profitable expenditure upon advertisement for the volume of output which they represent. In all other cases the long-period cost curve will be somewhat higher, incorporating in each short-period cost curve the cost of changing prices (since, by definition, only one price change can take place in connection with one size of plant). Now if the efficiency of advertisement is greater at lower than at higher prices, it follows that any of these long-period curves, with the exception of the limiting case, will slope downwards to the right more steeply than they would otherwise have done. Consequently we have here a case in which the oft-repeated cry of the advertising profession, that advertisement reduces prices, is demonstrably true. Its truth, however, in this connection, is confined to those cases in which the efficiency of advertisement increases as prices fall, and to the somewhat peculiar conditions of Mr. Harrod's long-period cost curve, in which each change in price is accompanied by a change in the scale of the plant as well as a change in the volume of output.

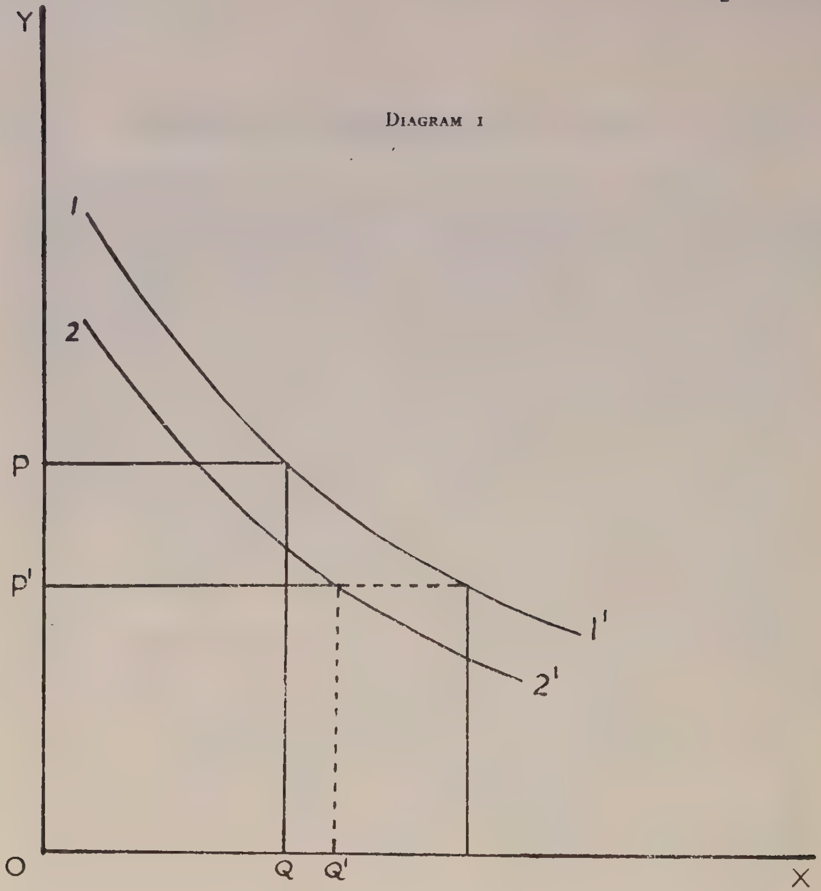

DIAGRAM I

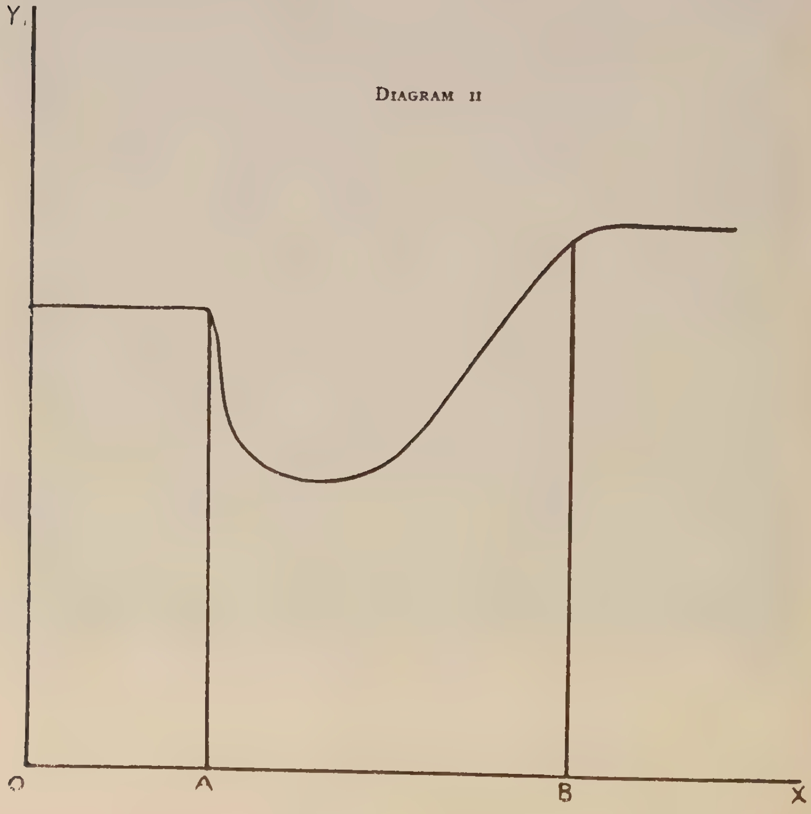

DIAGRAM II

The Influx of Labour into London and the South East 1920-36

By BRINLEY THOMAS

I

THE STATISTICAL MATERIAL[1]

The accidents of administrative practice can provide fruitful opportunities for statistical analysis. It happens that one of the items of information to be found on an unemployment book is the name of the Employment Exchange at which the book was originally issued. In July every year all unemployment books are collected by the Exchanges and are afterwards deposited in the Claims and Record Office of the Ministry of Labour at Kew. Naturally the new books sent out must reproduce necessary particulars from the old ones which they replace. Since 1920 the benefit and contribution records for every insured person in the country have been filed at Kew under the Office which issued his book: hence the administrative significance of the "Office of origin". This means that wherever a person may have migrated since he first became insured, his unemployment book will still bear the name of the Exchange which issued it to him in the first instance. This being so, it is possible at the annual exchange of the books to discover a great deal about the movement of insured workpeople from one part of the country to another.

Through the courtesy of the Ministry of Labour a Return was furnished by the South East Division, including London, in 1932 on the basis of the exchange of books in July of that year. The results of that inquiry were published in a previous issue of this journal.[2] Since 1932 the country has experienced a recovery from the trough of the depression; and increasing interest is being shown in the changing location of industrial activity. It seemed desirable to repeat the analysis in 1936; and the Ministry of Labour again kindly consented to supply a " Division of Origin " Return relating to the South East Division.[1] It covers all the Offices, 228 in number, and shows how many of the unemployment books current in July, 1936, had originally been issued in other Divisions, i.e., the South West, Midlands, North East, North West, Scotland, Wales, and in Northern Ireland. These are termed " foreign " books: at some time or other between 1920 and 1936 their owners must have crossed the boundary line separating the South East Division from the rest of the country. The new Return has a double significance: it reveals the volume and direction of the inflow of insured labour into London and the South Eastern Counties between 1920 and 1936—the long run trend; and, by comparing the 1936 figures with those recorded in 1932, we are able to gauge the influence of the upward swing of the trade cycle on the rate of absorption and the regional distribution of " immigrant " labour.

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[1] Assistance in the computing involved in this paper was provided by the Economic Research Division of the London School of Economics.

[2] See my article, "The Movement of Labour into South East England, 1920–32," ECONOMICA, May, 1934, pp. 220–41.

The 1936 Return is about as comprehensive as it can be made, for it gives the number of " foreign " books exchanged in each of the 228 Offices in the South East Division during the four weeks ended 27th July, 1936. The information received in 1932, on the other hand, excluded Offices with an insured population of under 3,000, so that the number supplying returns was 114. All comparisons between 1936 and 1932 will be based on these 114 Offices which comprised about 87 per cent. of the total books exchanged in the South East Division in July and August, 1936 (allowance having been made for books deliberately excluded).[2]

This brings us to the categories which, as in 1932, had to be left out. It was estimated that in 1936 there were current in the South East Division 70,000 unemployment books which had originally been issued direct from the Claims and Record Office at Kew to ex-members of the Regular Forces. These are in one sense " foreign " books but their inclusion would vitiate the present analysis. As they constitute something under 2 per cent. of the insured population of the

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[1] My thanks are due to the Ministry of Labour authorities for their continued interest in this inquiry and to the officials of the South East Division for the efficient way in which they presented the Returns. I take sole responsibility for the use and interpretation of the statistics.

[2] Only 108,000 books were exchanged in the South East Division during August compared with 3,830,000 in July. This shows that the great bulk of the work was completed in the first four weeks.

Division, a correction has been applied by reducing the number of books exchanged at each Office by this percentage. Furthermore, large London firms owning establishments in the provinces sometimes choose to dispatch all their employees' books in bulk to the Office nearest their headquarters. This practice results in the exchange of some "foreign" books in London, the owners of which live and work somewhere in the provinces. Bulk exchange of this kind took place at the following Offices: City of London (30,000), King's Cross (3,000), Edgware Road (3,000) and Camden Town (500). These were excluded from the "Division of Origin" Return. Thus, the rate of absorption, i.e., the "foreign" books expressed as a percentage of the total number of books exchanged, is calculated after making the above adjustments.

The area covered by the Return is the South East Division of the Ministry of Labour, as it was constituted up to 1st August, 1936.[1] The boundary line between London and the Counties of the South East is exactly the same as was adopted for the 1932 analysis.[2]

II

THE VOLUME AND DIRECTION OF THE INFLUX, 1920–36

The results of the 1936 Return may be summarised in the following Table:

"FOREIGN" BOOKS EXCHANGED IN THE SOUTH EAST DIVISION IN JULY, 1936

Area where Books were exchanged	No. of Books exchanged in July, 1936[3]	No. of Books "Foreign" to the S.E. Division	"Foreign" Books as per cent. of total exchanged per cent.
London	2,502,200	122,187	4.9
The Counties	1,221,400	96,853	7.9
South East Division	3,723,600	219,040	5.9

The total number of unemployment books exchanged in the South East Division in the four weeks ending 27th July, 1936, was 3,830,065. This covers all Offices, including Juvenile Employment Bureaux. Subtracting the 70,000 belonging to the Kew group and the 36,500 "bulk exchange" books, we obtain a corrected figure of 3,723,565. The number of these found to be "foreign" to the Division amounted to 219,040, thus yielding a rate of absorption for the South East of 5·9 per cent. London shows a total of 122,187 or 4·9 per cent. and the Counties 96,853 or 7·9 per cent. The absorption rate for the Counties is much higher than that of London. It is to be remembered that these "foreign" books may have entered the South East at any time between 1920 and 1936, and there is no means of finding out when they arrived. We are obviously underestimating the actual extent of the inflow of the movement, for some of the migrants have doubtless travelled back and forth more than once during the period. Moreover, juveniles may move in with their families and take out insurance books only after they settle down. There is reason to believe that most of the migration took place in the decade 1926–36.

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[1] On that date London became an independent Division, and the remainder is now the South East Division.

[2] It is to be noted that London in this analysis excludes Bromley, Dartford, Erith, Kingston, Harrow and Sutton, though they were included by the Ministry of Labour in the old administrative area of London. The boundaries of London in this inquiry roughly correspond to those of the Administrative County.

[3] Allowance has been made for centrally exchanged and "Kew" books.

It is of interest to compare our result with the calculations made by the Registrar General for the period 1921–35.

THE NET INFLOW OF POPULATION INTO THE SOUTH EAST.

Area	Net Gain by Migration.			Estimated Mid-Year Population in 1935.	Volume of Net Immigration 1921-35 as percentage of Mid-Year Population in 1935.
	1921–31	1931–35	1921–35		
Greater London ..	210,000	139,000	349,000	8,474,900	4·1
The Counties	405,000	231,000	636,000	5,533,600	11.5
The South East[2] ..	615,000	370,000	985,000	14,008,500	7.0

The Registrar General's South East does not coincide with that of the Ministry of Labour. Both comprise twelve counties (including London); but the place of Oxfordshire and Hants in the former is taken by Cambridgeshire and Norfolk in the latter. Greater London, too, in the above Table is larger than the London used in this inquiry. These discrepancies, however, are not sufficiently important to prevent us from comparing the two sets of figures. There

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[1] Registrar General's Annual Returns, Civil Report for 1935 and *Census of England and Wales, Preliminary Report*, 1931, p. XIII.

[2] The South East covers the counties of Bedford, Berks, Bucks, Essex, Hants, Herts, Kent, London, Middlesex, Oxford, Surrey, Sussex.

is a remarkable similarity between the " rates of absorption " shown in the two Tables, both in regard to their absolute and relative values. The immigrant proportion of the resident population in the South East is 7.0 per cent. according to the Registrar General's figures and 5.9 per cent. on the basis of unemployment insurance data. " Net gain by migration," of course, takes into account a two-way traffic of migrants. Those people who moved out of Greater London into the Counties are part of the inflow which has produced the high proportion of 11.5 per cent. compared with our rate of 7.9 per cent. ; and the loss of these people to Greater London helps to explain its relatively low " absorption rate " of 4.1 per cent. as against 4.9 per cent. in the present analysis. This factor of transference between the Metropolis and the Counties is absent from the Ministry of Labour data, for, by definition, a " foreign " book is one which originated somewhere outside the South East Division ; and we are dealing with a one-way traffic only. Moreover, the unemployment insurance statistics exclude dependants, uninsured persons and immigrants from abroad.

The contributions of the other six Divisions and Northern Ireland to the total inflow are set out in the following Table :

Area where Books were exchanged	Division of Origin (1936 Return).							Total "Foreign" Books.
	South West	Midlands	North East	North West	Scotland	Wales	Northern Ireland	
London	18,283	14,255	31,645	18,591	14,429	23,536	1,448	122,187
The Counties ..	15,967	12,591	26,937	15,168	8,456	17,118	616	96,853
South East Division ..	34,250	26,846	58,582	33,759	22,885	40,654	2,064	219,040

Of the total of 219,000 migrants, 27 per cent. came from the North East, 19 per cent. from Wales, 15 per cent. from the North West, 15 per cent. from the South West, 12 per cent. from the Midlands and 10 per cent. from Scotland. The proportions of the newcomers from the South West and the Midlands who have settled in the Counties are naturally higher than in the case of the others, for these are border Divisions and some of the recorded immigration is doubtless short distance movement over the boundary. It is clear from the distribution of the inflow between London and the Counties that the journey's end for most Scotchmen is

London.[1] The North East and Wales have contributed nearly a half of the total number of migrants.

An important side-light on these figures may be obtained by expressing the flow of labour as a proportion of the insured population of the area supplying it. In the following Table the average insured population in the period 1927–36 is taken as the appropriate base.

Division of Origin	Average Insured Population 1927–36	No. of "Foreign" Books in S.E. Division	Migration to S.E. per 1,000 of Average Insured Population 1927–36	Migration to S.E. per 1,000 of the No. in employment in June, 1936
Wales	607,000	40,654	67	96
South West	885,000	34,250	39	39
North East	2,044,000	58,582	29	34
Scotland	1,323,000	22,885	17	20
North West	2,156,000	33,759	16	19
Midlands	1,871,000	26,846	14	15
Northern Ireland	265,000	2,064	8	9
Total	9,151,000	219,040	24	27

Wales is outstanding in that almost 7 per cent. of its insured population has been absorbed by the South East ; and the bulk of this movement has probably occurred in the last decade. If it were possible to compute this export of labour as a proportion of the industrially active age-group 16–50 in Wales, the magnitude of the loss would be better appreciated. An approximation to this is to measure the outflow as a proportion of the estimated number of insured persons in employment in the Division in June, 1936. Figures for each Division are given in the last column of the above Table. We find that there were about 10 Welshmen in the South East for every 100 who were at work in the Principality in the middle of 1936. If those who have been absorbed by the Midlands and other areas could also be taken into account, it would no doubt appear that the number of Welshmen now in other parts of the country are a substantial fraction of the number at present working in Wales.

The comparatively high index for the South West is partly explained by the fact that it is a border Division ; further light on this will be available when the location of the migrants within the South East is analysed in more detail. The relative movement out of the North East is less than half that from Wales but about double that from the North West. Scotland stands slightly higher than the North West but well below the North East, one explanation for which is obviously the factor of distance. It is surprising that relatively so few have left Lancashire: perhaps its overflow has been mainly into the prosperous Midlands. Apart from the case of Wales, the index of the relative exodus based on the number in employment in 1936 does not show any striking deviations; and the order of the Divisions is unaffected.

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[1] This is a marked corroboration of a tendency commented upon in the previous article.

III

The Trend Between 1932 and 1936

The two Returns, in 1932 and 1936, of the number of "foreign" books exchanged in the South East are somewhat similar to the detailed Birthplaces statistics which used to be published in the Population Census.[1] Our data could be described as "the number of unemployment books enumerated in the South East but born elsewhere"; and the two Census dates are July, 1932, and July, 1936. To form a fairly accurate estimate of the number who actually moved into the Division in this period, it would be necessary to allow for the "rate of wastage," i.e., through death, retirement, exit from insurance, etc., among those enumerated in 1932. It is impossible to do this; but we shall not be far wrong if we treat the simple difference between the "foreign" totals at the two dates as measuring the increase in the amount of labour absorbed.

Some of the immigrants enumerated in a given Exchange area in 1932 may have returned to their original homes and their places in that area may have been filled by people from other Divisions who were already elsewhere in the South East in 1932. In this case the figures for that Office would reveal no change, whereas actually there would over the period have been emigration from the area to other Divisions. We know nothing definite about the movements of "foreign" books within the South East, and we cannot discover in detail what has been happening within each Exchange area. The number of books collected at an Office may increase merely because one or two large firms decide that it is administratively more convenient for them to exchange their employees' books there than at the Office which they previously used. A change-over of this kind introduces a purely artificial element into the statistics of the "industrial populations" of the separate Exchange areas.[1] The differences between the total number of books exchanged in 1932 and 1936 in the case of individual Offices must, therefore, be carefully scrutinised, so that artificial causes may as far as possible be eliminated. In order to minimise the effect of such extraneous factors, we shall merge contiguous Exchanges into regional groups, for, then, the artificial gains of one Office are likely to be offset to some extent by the corresponding losses of other Offices in the group. A comparison of such regions will throw some light on changes in the location of immigrants since 1932.

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[1] This useful section of the Census was dropped as a result of the "Geddes Axe" in 1921. The preparations for the 1931 Census were also carried out under the shadow of an axe; and there was no hope for the inclusion of what the authorities mistakenly regard as statistical luxuries. We may well wonder what conditions will be like in 1941.

For the Division as a whole the local transfers cancel out, and a positive difference between the total of "foreign" books in 1932 and 1936 represents a genuine increase in immigration. This part of the inquiry is necessarily confined to the 114 Offices which rendered returns in 1932: these were responsible for 205,000 out of an aggregate of 219,000 "foreign" books exchanged in 1936.

We observe from Table I on page 335 that the total of "foreign" books exchanged in the South East (the 114 Offices) rose from 129,146 in 1932 to 205,449 in 1936. The increase in the rate of absorption was most marked in the Counties, namely, from 6.3 per cent. to 8.7 per cent. The inflow has been very rapid in the last three or four years. Over one-third of the total volume of immigration into the South East registered in 1936 is to be attributed to the four years 1932–36. Comparing the definitive total of 133,306 "foreign" books in 1932 with the 1936 figure,[2] we find that the addition of 72,000 was divided fairly equally between London and the Counties, as may be seen from Table II. Only relatively few extra migrants have come from the Midlands and the South West since 1932: the North East alone is responsible for nearly a third of the increase. A comparison of the proportionate growth in migration from different areas reveals interesting variations. The influx from Northern Ireland is not numerically important; but most of it has taken place since 1932. Of the 21,900 insured Scotch people enumerated in the South East in 1936 over a half appear to have come in during the previous four years. The movement from the North East and Wales had been quite considerable up to 1932: it has continued unabated, the number of North Eastern and Welsh immigrants in the South East being respectively 75 per cent. and 61 per cent. greater than in 1932.

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[1] A good example of such an administrative change (which, fortunately for this analysis, is very rare on this scale) is found in the Edgware Road Exchange. In 1936 the London Passenger Transport Board exchanged in bulk 65,000 books there, whereas in 1932, as the Board was not in existence, the books of the London Transport workers were exchanged in several Offices, particularly, Westminster, Borough and Redhill.

[2] See the footnote to Table II.

We will now examine how the industrial recovery has affected the distribution of labour over different areas within the South East. For this purpose the Division has been divided into 12 Regions, the composition of which is given on page 336.[1] Table III shows the number of books exchanged together with the "foreign" books among them in each Region in 1932 and 1936; and the Regions are listed in the ascending order of their rate of absorption in 1936. They are the Central Boroughs, Inner Boroughs, North (Lea Valley), Outer East, South East Towns, Seaside Towns, South East Suburbs, Country Towns, Outer South West, North West, Outer West, Kent Coalfield.

There have been some striking developments during the four years 1932–6. The trend of the influx of outside workers has been into those areas which had the highest rates of absorption at the former date. The proportion of "foreign" books exchanged in the Central and Inner Boroughs is only slightly above what it was in 1932. In spite of the boom the attracting power of a large part of the County of London has remained practically unchanged. On the other hand, the North West and especially the Outer West of London have been importing labour at a very rapid rate. The proportion of all immigrants found in the two Central Regions fell from 43 per cent. in 1932 to 35 per cent. in 1936, while in the Outer West and North West Regions the proportion went up from 18 per cent. to 21 per cent. Other areas where there has been a substantial rise in the rate of absorption are the Outer South West, the Country Towns and the South East Suburbs: their combined proportion of all "foreign" books rose from 17 per cent. to 20 per cent. The South East Towns and the Lea Valley also show large proportionate increases in their rate of absorption. Well over a half of the growth in the Outer East is due to Dagenham where the number of "foreign" books rose from 3,958 to 5,743.

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[1] I am indebted to my friend, Mr. M. I. Michaels, for several valuable discussions and for his advice on the problem of dividing the South East into appropriate Regions.

Another way of describing these changes is to inquire how the increase of "foreign" books since 1932 has been spread over the Regions. The 12 areas together absorbed an additional total of 71,000 during the four years. No less than 50 per cent. of this increase is to be attributed to the North West, Outer West, the Outer South West and the Country Towns; 22 per cent. of it was taken by the two Central areas, 15 per cent. by the Eastern districts and 12 per cent. by the Lea Valley, the Seaside Towns and the Kent Coalfield.

It is worth while examining more closely the remarkable industrial development in the North West and Outer West sectors. The following Table gives particulars of the Exchange areas in these Regions:

Region	Exchanges	"Foreign" Books		Increase in "Foreign" Books	Rate of Absorption	
		1932	1936		1932	1936
North West	Willesden	3,597	6,026	2,429	8.1	10.1
	Acton	2,838	4,802	1,964	8.2	9.4
	Hendon	3,066	5,293	2,227	13.1	13.1
	Harrow	2,537	4,489	1,952	13.0	17.4
	St. Albans	1,025	2,124	1,099	10.5	14.9
	Watford	1,821	4,819	2,998	8.6	18.9
	Total	14,884	27,553	12,669	11.4	13.4
Outer West	Brentford	1,395	1,939	544	6.6	7.7
	Hounslow	1,388	2,053	665	10.5	12.0
	Southall	2,969	2,649	−320	11.4	15.1
	Hayes[1]	—	3,103	3,103	—	17.1
	Uxbridge	700	1,576	876	10.0	14.7
	Slough	1,940	4,551	2,611	15.6	23.1
	Total	8,392	15,871	7,479	10.9	14.4

That two-sevenths of the total increase in immigration was absorbed by these two areas is proof of the extent of the industrial activity there. Nearly one-seventh of their insured population has come in from other Divisions. The region stretching from Willesden to Watford and St. Albans has a larger concentration of industries than the Western area along the Great West Road out to Slough ; but the latter has shown the greater relative capacity for attracting labour from the rest of the country. Both stand out in striking contrast to the Northern Region covering Tottenham, Enfield, Wood Green and Finchley. There the rate of absorption is as low as 4.8 per cent. It is a flourishing industrial district, and larger than the Outer West (135,000 as against 110,000 in 1936). The chief reason for its low capacity for importing labour from other Divisions is probably to be found in the skilled character of the industries located there. For example, in recent years the furniture trade has been moving into the area away from Hackney and Shoreditch[1] ; and the workers have tended to follow the factories. Such an industry has a relatively low demand for unskilled or semi-skilled labour. In the West and North West, on the other hand, we find a vast collection of light industries where mass production is prevalent. Considerable expansion has taken place in industries such as motor vehicles, cycles and aircraft, electrical engineering, electrical cables and apparatus, cardboard boxes, stationery, paper and paper boards.

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[1] The Hayes Office did not exist in 1932; the work was done by Southall.

Another interesting feature of Table III is the surprising extent to which the region described as "Country Towns" has received immigrant labour. Between 1932 and 1936 the number of "foreign" books exchanged at Bedford went up from 914 to 2,633, at Norwich from 1,187 to 2,122, at Luton from 2,648 to 6,552. Quite substantial increases were also recorded at Welwyn, Aylesbury and Letchworth. No less than half the immigrants enumerated in the country towns taken together in 1936 came in during the previous four years. By contrast the group of seaside towns has not offered by any means as much scope : the increase in their total of "foreign" books is slight. The figures suggest that some of the new industrial developments are affecting the independent and sometimes distant country towns. The Outer South West, an area reaching as far as Guildford, comes out well in this analysis : it includes Kingston where there has been large aircraft development. As one would anticipate, the Kent Coalfield has the highest rate of absorption ; and since 1932 there appears to have been continued expansion in that area.

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[1] There is plenty of evidence for this in the annual *Surveys of Industrial Development* published by the Board of Trade.

The immigrant sections of the insured population of five of the Regions may now be analysed according to the Divisions from which the migrants came.

DIVISION OF ORIGIN (1936 RETURN)

Region	Total Immigrants[1]	Division of Origin					
		South West	Midlands	North East	North West	Scotland	Wales
		Percentages of Total					
Outer West	15,871	14	9	26	15	8	28
North West	27,553	9	9	34	13	10	25
North (Lea Valley) ..	6,460	12	14	30	16	11	17
Country Towns ..	21,179	10	17	28	19	10	15
Kent Coalfield ..	5,134	4	13	40	8	8	26
S.E. Division ..	219,040	15	12	27	15	10	19

Among the interesting points in this Table is the relatively high proportion of Midlanders in the North (Lea Valley) Region, which again suggests the greater scope for skilled workers in that area. The same appears to be true of the country towns, but the nearness of the Midlands may be the explanation in this case. Welshmen comprise 28 per cent. of the total in the Brentford-Slough district, while men from Northumberland, Durham and Yorkshire are the prominent section in the Hendon–Watford area and the Kent Coalfield. Immigrants from the South West of England are to be found mainly in the western parts of the Division. The country towns seem to draw their migrants fairly evenly from all Divisions, with a surprisingly high proportion from Lancashire and Cheshire.

This inquiry has shown that the redistribution of labour in favour of the South East of England has received a strong impetus in the last few years. It is too early to discover whether the rearmament programme and the steps being taken by the Government to encourage industry to settle in areas of heavy unemployment will have the effect of checking this movement. Many of the newcomers, especially if they have not removed their families or are unmarried, are naturally quite ready to return to their old homes as soon as there is a prospect of work. If capital were again to flow into the Special Areas, there would be little difficulty about getting a reverse movement of labour. It is, thus, all the more important that, if new industrial activity is to be stimulated in the depressed areas, it should not be a temporary dose of armaments work but investment which has a reasonable chance of outlasting the present boom.

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[1] Including the few from Northern Ireland,

TABLE I

THE SOUTH EAST DIVISION (114 OFFICES), 1932 AND 1936

Area	Date of Count	Total Books exchanged in July	No. of Books "foreign" to S.E. Division	"Foreign" Books as per cent. of total exchanged
London	1932	2,083,000	82,864	4.0
	1936	2,502,000	122,187	4.9
The Counties ..	1932	738,000	46,282	6.3
	1936	952,000	83,262	8.7
South East Division .. (114 offices)	1932	2,821,000	129,146	4.6
	1936	3,454,000	205,449	5.9

TABLE II

DIVISION OF ORIGIN OF "FOREIGN" BOOKS EXCHANGED IN THE SOUTH EAST DIVISION (114 OFFICES), 1932 AND 1936

Area	Date of Count	Division of Origin							Total "Foreign" Books
		South West	Midlands	North East	North West	Scotland	Wales	Northern Ireland	
London	1932	15,442	13,037	20,071	12,980	8,220	15,916	577	86,243
	1936	18,283	14,255	31,645	18,591	14,429	23,536	1,448	122,187
	Inc.	2,841	1,218	11,574	5,611	6,209	7,620	871	35,944
The Counties	1932	9,374	7,711	11,491	7,457	2,681	8,173	176	47,063
	1936	12,451	10,264	23,563	13,669	7,463	15,294	558	83,262
	Inc.	3,077	2,553	12,072	6,212	4,782	7,121	382	36,199
South East Division (114 offices)	1932	24,816	20,748	31,562	20,437	10,901	24,089	753	133,306[1]
	1936	30,734	24,519	55,208	32,260	21,892	38,830	2,006	205,449
	Inc.	5,918	3,771	23,646	11,823	10,991	14,741	1,253	72,143
	1936	30,734	24,519	55,208	32,260	21,892	38,830	2,006	205,449
	1932 = 100	124	118	175	158	201	161	266	154

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[1] This total of 133,306 "foreign" books for 1932 is larger than the figure for that year given in Table I because it includes "foreign" books exchanged after July 23rd, 1932, at certain Offices which had not completed their work at that date. For the purpose of calculating rates of absorption for 1932 it was safer to consider all Offices according to the number of books they had dealt with up to July 23rd. But, in examining the migration from the point of view of the contributing Divisions, it was legitimate to use the larger total of "foreign" books.

TABLE III
REGIONAL ANALYSIS
THE SOUTH EAST DIVISION

Region	Total Books exchanged [1]		"Foreign" Books [2]		Rate of Absorption	
	1932	1936	1932	1936	1932 per cent.	1936 per cent.
Central Boroughs ..	983,700	1,187,000	38,004	47,870	3.8	4.0
Inner Boroughs ..	492,600	554,000	17,800	23,382	3.9	4.2
North (Lea Valley) ..	97,400	134,800	3,406	6,460	3.5	4.8
Outer East	217,500	256,000	9,901	13,018	4.5	5.1
South East Towns ..	151,900	189,600	5,112	9,955	3.4	5.2
Seaside Towns.. ..	170,200	202,200	7,843	10,761	4.6	5.3
South East Suburbs	84,200	111,600	4,343	7,561	4.9	6.8
Country Towns ..	196,500	257,000	10,683	21,179	5.4	8.0
Outer South West ..	111,200	142,900	7,410	12,895	6.7	9.0
North West	142,300	220,200	14,884	27,553	10.5	12.4
Outer West	77,000	110,300	8,392	15,871	10.9	14.4
Kent Coalfield ..	16,200	27,200	2,876	5,134	17.7	18.8
Total S.E. .. .. (99 Offices)	2,740,700	3,392,800	130,654[3]	201,639	4.8	5.9

[1] For 1932 the figures represent books exchanged up to 23rd July minus centrally exchanged and "Kew" books. The same applies to 1936 except that the end of the count was 27th July.

[2] Fifteen small Offices have not been allocated to Regions. The number of "foreign" books thus excluded is 2652 in 1932 and 3810 in 1936.

[3] All "foreign" books in the 1932 count and not only those exchanged up to 23rd July are included, so as to bring the 1932 figures more into line with the Return for 1936, when the closing date was July 27th, and a greater proportion of all books than in 1932 appear to have been exchanged in July. This explains why the general rate of absorption for 1932 given here is slightly higher than in Table I and in the previous article.

COMPOSITION OF THE REGIONS

Region	Exchanges
Central Boroughs ..	Great Marlborough Street, Camden Town, King's Cross, Edgware Road, Stepney, Borough, Bermondsey, City, Westminster, Shoreditch, London Building Trades.
Inner Boroughs ..	Battersea, Walham Green, Deptford, Brixton, Camberwell, Hackney, Holloway, Lewisham, Poplar, Shepherd's Bush, Tooting.
North (Lea Valley) ..	Tottenham, Enfield, Wood Green, Finchley.
Outer East	Barking, Canning Town, East Ham, Ilford, Leyton, Stratford, Romford, Dagenham.
South East Towns ..	Chatham, Maidstone, Tonbridge, Dartford, Erith, Gravesend, Grays, Tilbury, Woolwich.
Seaside Towns	Brighton, Eastbourne, Folkestone, Great Yarmouth, Hastings, King's Lynn, Lowestoft, Margate, Ramsgate, Southend, Worthing, Newhaven.
South East Suburbs ..	Bromley, Croydon, Penge, Sutton, Orpington, Sidcup, Caterham.
Country Towns	Luton, High Wycombe, Aylesbury, Welwyn, Chelmsford, Bedford, Colchester, Letchworth, Braintree, Cambridge, Norwich, Ipswich, Wolverton.
Outer South West ..	Richmond, Staines, The Dittons, Kingston, Wimbledon, Redhill, Leatherhead, Woking, Guildford, Epsom, Weybridge.
North West	Willesden, Acton, Hendon, Harrow, St. Albans, Watford.
Outer West	Brentford, Hounslow, Southall (including Hayes)[1], Uxbridge, Slough.
Kent Coalfield	Canterbury, Dover (including Deal)[1].

[1] Hayes and Deal were not in the 1932 Return; but are included in 1936 because they now do part of the work formerly done at Southall (in the case of Hayes) and Canterbury and Dover (in the case of Deal).

Do Synthetic Propositions *a Priori* Exist in Economics

A Reply to Dr. Bernardelli

By Felix Kaufmann

I am very pleased to accept the opportunity offered to me to reply to the article from the pen of Dr. Bernardelli which appeared in the November, 1936, number of this journal (pp. 443–54) under the title : " What has Philosophy to contribute to the Social Sciences and to Economics in Particular ? "

The article in question is intended as a review of my " Methodenlehre der Sozialwissenschaften " and Dr. Stonier's " Der logische Charakter der Sozialwissenschaften ". Dr. Bernardelli's thesis was that " the aid which philosophy can give to an empirical science can be determined as the task of collecting, systematising and justifying " synthetic propositions *a priori* (p. 447). His criticism of the two books was, accordingly, that they did not solve this task satisfactorily.

In the following I shall refer to the chief points in this criticism only in so far as they concern my book, and state my attitude to the problem of synthetic propositions *a priori*.

The propositions of geometry have been held from Plato onwards to be the model of such propositions ; hence it is easy to understand that, on the basis of the view which we have already quoted, Dr. Bernardelli finds it necessary to protest against my attack on the theory that the propositions of geometry are synthetic propositions *a priori*. I quote his remarks on this point *in extenso*. " The relevance of this remark will be illustrated by the following example : Dr. Kaufmann attacks Kant's assertion that the geometrical propositions are of the synthetical kind (p. 311) and he assures his readers that this Kantian doctrine received a death-blow through the discovery of non-euclidean geometries (p. 54). This story, which is rarely missing in any tract on natural philosophy and which is thus backed by many a good authority, is, however, a fairy tale and not even a plausible one. The possibility of non-euclidean geometries is proved by demonstrating that no logical contradiction will occur in building up geometries on the basis of principles (axioms) which are the logical contrary of Euclid's axioms. Thus the axiom that the angular sum in a plane triangle amounts to 180 degrees may be substituted by the proposition that it amounts to more or to less than 180 degrees. Hence, instead of being a death-blow to the Kantian doctrine the discovery of the non-euclidean geometries is only explicit proof of what he said. It is of the very essence of his teaching that geometrical axioms are indeed no "necessities of thought" in the sense that their negation would be contrary to logic but that they are synthetic propositions in the genuine sense of the word."

It is true that there are a number of writers who maintain the view which Dr. Bernardelli has criticised, but I am certainly not one of them. The sentence in my book which Dr. Bernardelli refers to runs as follows: "But this Kantian doctrine which identifies geometry with euclidean geometry was already shaken (geriet schon ins Wanken) when it was shown that the so-called non-euclidean geometries (that is, those for which the euclidean axiom of parallels does not hold) were self-consistent. The death blow to this doctrine was dealt by Poincaré's conventionalist explanation of geometry and finally by Einstein's general theory of relativity which demonstrated that the non-euclidean geometry of Riemann was better suited to describing the physical world than the euclidean geometry." It will be seen that I never maintained that the discovery of the non-euclidean geometries dealt a death-blow to Kant's theory. On the other hand the statement that Kant's theory was shaken by the discovery of the non-euclidean geometries is a historical fact, since the construction of such systems led to investigations as to whether the euclidean geometry really was the only one which was capable of describing nature. The first step on this path was Gauss' empirical investigation as to whether the sum of the angles of a large terrestrial triangle really was 180 degrees, and at the end are to be found the decisive results obtained by H. Poincaré, Minkowski and Einstein.

This discovery of the non-euclidean geometries is certainly not, as Dr. Bernardelli affirms, "explicit proof of what Kant said". It only shows that Euclid's postulate is not an analytical statement and that it is not derivable from the other axioms of geometry. However, it does not show that this postulate is valid for all physical experience, and indeed it was clearly demonstrated by Einstein's General Theory of Relativity that this is not the case, since Einstein makes use of the calculus of a non-euclidean geometry, that of Riemann.[1]

As has been shown by the Neo-Kantians (Cohen), Kant held that euclidean geometry was a " scientific fact " which simply could not be denied, because it was so closely connected with Newton's physics which at that time seemed to be beyond all doubt. In the meantime this view has been abandoned.

The general rule which this line of thoughts leads to, runs as follows : if we understand by the phrase " synthetic propositions *a priori* ", principles which, without being based upon experience, can lay down certain rules about experience, then there are no such propositions. The remark " that any experience presupposes already certain principles *a priori* as the very condition of its possibility " (p. 448) may easily lead to misunderstandings (which, in my opinion, Fries and Nelson, on whom Dr. Bernardelli bases his argument, have not been able to avoid[2]). The careful and correct interpretation of this statement is : Each empirical statement and *a fortiori* a system of such statements called science is based on general assumptions which the scientist accepts, frequently unconsciously, without further examination. It is one of the most important tasks of methodology to make them explicit, to " collect, systematise and justify " them. So far, therefore, I have nothing to object to in Dr. Bernardelli's argument. But I find it quite impossible to go with him when, following the tradition of Fries's school, he claims for these assumptions a special " necessary " validity, which would be required if the use of the phrase " synthetic propositions *a priori* " were to be justified. This position is untenable, and in my opinion it has not even the right to appeal for support to the authority of Kant.[3] The chief danger of this doctrine is that it leads to a confusion between empirical statements and definitions and thus to begging the question.

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[1] A more detailed exposition of the character of geometrical propositions and concepts is to be found in my book : *Das Unendliche in der Mathematik und seine Ausschaltung*, Vienna. 1930, p. 111ff.

[2] Apart from this I agree entirely with Dr. Bernardelli that the work of Fries and Nelson is worthy of far more attention than it has yet received.

[3] This point, which would lead to an excursus into the history of philosophy to explain the change which Kant's chief ideas have been subjected to on the part of Fries's school, cannot, of course, be considered here.

Here we shall seek to make clear the essence of this argument by examining the example of economic behaviour. This example may be considered as follows: After having analysed the propositions of Economics which we take for granted, we declare that the economic behaviour of men has the properties $p_1, p_2, \ldots p_n$. These properties are held to be necessary properties, in the sense that when one of them is absent we say that we are not dealing with economic behaviour. This statement looks like a judgment about reality and gives the appearance of containing an imputation of certain given properties to the real fact of economic behaviour. But that is a mistake; all that is done is to establish the definition of the concept " economic behaviour ". The proposition says, in fact: we call human behaviour economic behaviour, only when it has the properties $p_1, p_2, \ldots p_n$. If, however, the concept of economic behaviour is defined independently of the properties $p_1, p_2, \ldots p_n$, and it is then maintained that economic behaviour has the properties $p_1, p_2, \ldots p_n$, then we really do have an empirical statement which can be verified by experience or, may be, shown to be false. Thus, if the definition of economic behaviour is made without reference to the concept " behaviour according to the economic principle ", it is possible to ask whether economic behaviour does in fact accord with this principle as a rule. On the other hand, when economic behaviour is defined as being " behaviour in accordance with the economic principle ", such a question is meaningless. But to speak in this case of the necessary properties of economic behaviour would give rise to the false impression that relations between real factors were in question, whereas, in fact, it is merely a matter of defining concepts.

The analysis of the marginal utility principle which I have given in my book is based on this distinction, which is certainly no very abstruse one, though it appears to be one which, unfortunately, is very fruitful for methodological criticism in the social sciences. In this analysis I show that three different meanings have been confused in the interpretation of this principle.

(1) If, as is often done, we think of the marginal utility principle as being deduced from the concept of economic behaviour, then this principle is not an empirical statement. It is contained in the definition of the concept " rational valuation of a unit of a given stock of a commodity ". Thus,

like all definitions, it cannot be refuted since it makes no statement about reality. It can neither be proved nor shown to be false.

(2) On the other hand we have the empirical question, whether, or to what extent the concept of rational economic behaviour and the marginal utility principle are capable of being used to describe actual economic phenomena. The statement that this is possible is an assumption about reality and therefore can be subjected to proof and may be shown to be false.

(3) The other proposition which is also often termed "marginal utility" principle runs as follows: within any given period (to which the economic plan of an individual must be limited) an individual will only derive the same utility from a limited number of units of one type of commodity and for any subsequent unit the utility will be less. Thus the marginal utility of the unit of the commodity falls when the number of units which the individual has at his disposal exceeds a given number. This is a proposition derived from experience and is supported by the fact of the satiation of needs which we can verify by introspection (Gossen's first law).

There is no relation between the first two of these propositions and Gossen's law. Most of the pseudo-problems concerned with the validity of the marginal utility principle arise from the fact that, as a result of the confusion between the three meanings which we have mentioned above, we get the impression that the marginal utility principle is an *a priori* proposition which all the same could make some statement about reality.

After studying Dr. Bernardelli's critical remarks concerning this argument as it is to be found in my book I was not at all sure that he had completely understood the point of view which has been sketched out above. Dr. Bernardelli has brought forward no precisely formulated objection to anything which I said; instead, I am taken to task for not following another line of thought. The course which he thinks I should have taken is that which he has followed in his interesting work, "Die Grundlagen der ökonomischen Theorie",[1] where he seeks to explain the economic principle with the help of the methods of Fries's school.

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[1] Tübingen, 1933. This book seems to me to be a good and careful work which is well suited as a basis for the discussion of the questions which it deals with. Its results are subject to the objections which have been mentioned above against Dr. Bernardelli's views regarding synthetic propositions *a priori*.

A clear distinction between definitions and empirical statements also enables us to avoid the misunderstandings which are associated with the concept of " the least important need " as it is used in the writings of the marginal utility school. On the one hand we can define the " least important need " as that which the individual did in fact renounce when he was obliged to sacrifice the satisfaction of one of his needs. In this case it would not be entirely wrong to say that the individual will always renounce the satisfaction of the least important need, though this form of expression is certainly not to be recommended. In this case, however, we have said absolutely nothing about economic reality. On the other hand, we have a second type of definition where the " least important need " is defined in terms of factors which are independent of the behaviour acts which the concept is to be used to explain. If " the least important need " is defined in this manner we can make the empirical statement that the economic behaviour of the individual will involve renouncing the satisfaction of this need. This statement can be examined and it may be shown to be wrong.

In the theories of the marginal utility school, whenever the concept of the least important need is used as a means of explaining economic reality it must, of course, be defined according to the second group of definitions. Otherwise any explanation of economic behaviour by referring it to the least important need would involve begging the question. But when we make use of a definition of the second type, we cannot claim any absolute validity for the proposition that economic behaviour always involves renouncing the satisfaction of the least important need.

The British Exchange Equalisation Fund, 1935-1937

By F. W. Paish

After this article had already been planned, the justification for its publication was reduced by the partial lifting of the veil of official secrecy with which the working of the British Exchange Equalisation Account has been surrounded since its inception five years ago. Since, however, the Chancellor's statement has so far revealed the gold holding of the Fund for a single date only—March 30th, 1937—it is thought that it may be of interest to trace its movements since the end of 1935, when the previous estimates[1] terminated. Encouragement to proceed with publication has been derived from the fact that, although the estimates and calculations used have been in no way altered since the publication of the official figure for March 30th, the estimate for that date is within some 500,000 ounces, or about £3½ Mn., of the true figure. This is, of course, a fortunate accident. The smallest range of error which could in general be hoped for in these estimates is of the order of plus or minus 3 Mn. ounces, or about £20 Mn., while at times the error has no doubt been far greater than this. Nevertheless, it cannot but be gratifying to the author that the Chancellor should have chosen a date for his first revelation when the actual figure was so close to the estimate. It is to be hoped that, in addition to the publication of future half-yearly figures, three months in arrear, the Treasury will publish the figures of the Fund for the years since its inception, and thus make it possible to effect the necessary corrections in the earlier estimates.

The assumptions and methods of calculation used in these estimates of the movements of the Exchange Fund have been fully described in the two previous articles on this subject,[1] and it is not necessary to repeat them here. One important departure from the assumptions formerly used has now been made. Previously it was assumed that the total of "tap" Treasury Bills held otherwise than by the Exchange Fund remained constant at £145 Mn. This assumption has now been abandoned. Since November, 1935, no less than three medium-term loans have been issued, of which large portions are believed to have been taken up by Government Departments and the Bank of England, and thereafter sold off gradually at declining prices. It is further believed that the Bank and Departments obtained a good deal of the money required to meet the calls on these loans by reducing their holdings of "tap" Treasury Bills, which were subsequently replenished, partly out of new resources, partly out of sales of the loans taken up, and on occasions partly out of the proceeds of Treasury Bonds repaid on maturity. These beliefs find considerable confirmation in the changes in the total of "tap" bills on the dates of the calls on the various loans and of repayments of maturing Bonds.

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[1] Economica, February, 1935, and February, 1936.

In estimating the total of "tap" bills held otherwise than by the Fund, account has been taken of probable changes due to these requirements. In addition, account has been taken of the presumed effects of the reduction of £60 Mn. in the Bank of England's Fiduciary Issue in September, 1936. This is assumed to have resulted in the transfer of £60 Mn. of Treasury Bills from the Issue Department to the Fund. The estimated figures of "tap" Treasury Bill holdings at various critical dates are as follows :—

			Total "Tap" Bills.	Estimated holdings otherwise than by Fund.	Estimated holdings of Fund.
1935.	December	6th.	343	145	198
1936.	April	4th.	204	65	139
	"	18th.	228	90	138
	November	14th.	235	140	95
	December	12th.	205	115	90
	"	19th.	202	55	147
1937.	January	9th.	175	30	145
	April	24th	168	115	53
	June	26th.	129	85	44

The first date is just before the issue of the 2½ per cent. Funding Loan. The second is just after the payment of the final call. By April 18th, 1936, the final repayment of maturing Treasury Bonds, (£42 Mn.), had been made. November 14th was just before the issue of the 2¾ per cent. Funding Loan. December 12th was just before the reduction in the Fiduciary Issue and December 19th just after it. January 9th, 1937, was just after the final call on the 2¾ per cent. Funding Loan. April 24th was just before the issue of the 2½ per cent. National Defence Bonds, on which about £67 Mn. had been received by June 26th.

A second, and minor, change in the method of calculation is in the choice of an accounting unit. When these estimates were first started the unit used was the dollar. When the United States left the gold standard, the calculations were converted into francs, and it was in terms of francs that the two previously published estimates of the Fund's gold holdings were expressed. Now that the franc has also left the gold standard, the calculations are made in terms of fine ounces, and it is to be hoped that a unit has at last been found which is not subject to depreciation. To convert the franc totals given in the previous articles into terms of ounces it is necessary to divide them by about 528.

The estimates thus made of the movements since the end of 1935 in the Exchange Fund's gold holdings are shown in the Tables and Chart on pages 348 and 349. Of the three curves described on the chart, the lowest one represents the gold in the Fund's actual possession. The next shows both the gold in the Fund's own possession and its contingent share in the gold which it has sold to the Bank of England since June, 1932, at below cost price. The uppermost line shows the total acquired by the Fund and the Bank together since June, 1932. To estimate the total amount held by the Fund and the Bank together at any date it would be necessary to add to this top line an amount of 28·2 Mn. ounces, representing the Bank's gold reserve of £120 Mn. at the date of the inauguration of the Fund.

These estimates indicate that, apart from two temporary checks, the Fund and the Bank together have gained gold almost continuously since the end of 1935. The smaller of the two checks occurred between the middle of February, 1936, and the end of April. It may probably be ascribed to the effects of the first sterling credit of £40 Mn. granted by British banks to the French Government. The second and larger check lasted from September, 1936, till early in January, 1937. This is clearly due to the effects of the first French devaluation. The effects of the second sterling credit, granted in February–March, 1937, are hardly discernible.

The total increase in the combined gold holdings of the Bank and the Fund since the beginning of 1936 is shown as nearly 40 Mn. ounces, worth at present prices about £281 Mn. This is considerably larger than the whole of the increase estimated for the preceding three years and a half. The estimated total increase in the five years ending June, 1937, is about 78 Mn. ounces, worth nearly £550 Mn. The total gold holdings of the Fund and the Bank combined at the end of June would therefore be in the neighbourhood of £750 Mn.[1]

Another question of interest which arises in connection with the Exchange Fund is that of its cost and profits. In answer to a question in the House of Commons on July 1st, the Chancellor of the Exchequer stated that the amount of gold which had to be financed on behalf of the Fund on March 30th, 1937, was £187 Mn. held by the Fund plus the " loss " of £115 Mn. on the gold resold to the Bank. He calculated the interest cost on this £302 Mn. at the current Treasury Bill rate of 9/16 per cent. thus estimating the annual interest charge at £1·7 Mn.

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[1] This estimate is if anything too low. The most unsatisfactory part of the Chart is the section covering May and June, 1937. Although the Bank and the Departments are believed to have had to take up some 75 per cent. of the issue of National Defence Bonds, on which the total payments to the end of June amounted to £67 Mn., and although the Fund is believed to have bought gold heavily during the same period, the fall in the total of "tap" bills between May 1st and June 26th was only £24 Mn. Even allowing for heavy sales of part-paid National Defence Bonds by the Bank and Departments, and for some inflow of resources from elsewhere, the reduction in their bill-holdings can hardly be put at less than £25 Mn. This would mean that the Fund's holding of bills was virtually unchanged over the period, and their total purchases of gold would be limited to the £13 Mn. received from the re-sale of gold to the Bank. Even if allowance is made for possible re-sales of gold by the Fund to arbitrageurs for shipment to the United States, the figure of £13 Mn. seems much too low. One possible explanation may lie in the rumours that recently the Bank, or the Departments, or the Fund, have been re-selling "tap" bills in the market. This would effectively conceal the drop in the sellers' bill holdings until the bills matured, possibly three months later. In this case the concealed movement would show itself sooner or later in a fall in the "tap" bill total greater than could be explained in the light of contemporary developments—unless, of course, special steps were taken to continue the concealment.

Concerning this calculation two comments may be made, one with regard to the amount requiring financing, and the other with regard to the rate of interest used. Since June, 1932, when the Fund was inaugurated, the fiduciary note issue of the Bank of England has been reduced by £75 Mn., thus enabling the Bank to increase its gold holdings by £206 Mn. while expanding the note issue by only £131 Mn. As the securities held by the Bank against the fiduciary issue are in effect a loan to the Government free of interest, a reduction in the fiduciary issue obliges the Government to re-borrow a corresponding amount elsewhere and pay interest on it. It would therefore seem that, in calculating the amount of sterilised gold requiring finance, the reduction in the fiduciary issue should be included, raising the amount in question from £302 Mn. to £377 Mn.

On the second point, the Chancellor's reply appears to imply that, if it had not been found necessary to sterilise £302 Mn. (or more correctly £377 Mn.) of gold, the total of Treasury Bills in the hands of the market would have been that amount less than it is in fact to-day. But since the total of market bills on March 30th last was only £43 Mn. higher than it was five years earlier, this assumption implies that, but for the gold sterilisation policy, the total of market Treasury Bills would have fallen during the five years by £334 Mn., from £457 Mn. to £123 Mn. This assumption does not appear very probable. It is surely at least equally reasonable to assume that the total of Treasury Bills in the market would have been approximately maintained, and that a large proportion of the funding loans issued during the past five years would have been unnecessary. If this alternative assumption is used, it would appear that the rate of 9/16 per cent. should be applied to only £43 Mn. of the amount to be financed, and that the interest charge on the remainder should be calculated at something like 3 per cent. If this is so, the present annual interest cost of holding £302 Mn. of sterilised gold would be, not £1·7 Mn., but about £8·0 Mn., while if the amount to be financed is taken as £377 Mn., as suggested above, the annual interest cost would be about £10·2 Mn. On the same assumptions, the total interest cost of the Fund since its inception would probably be in the neighbourhood of £20 Mn. or £25 Mn.

The total profit of the Fund since June, 1932, is estimated at about £35 Mn., of which about £24 Mn. is the profit made upon the actual operations of the Fund, and about £11 Mn. is due to the subsequent appreciation of the gold which the Fund has sold from time to time to the Bank of England and in which it still holds the ultimate equity. The great bulk of these profits seem to have been made before the end of 1934; between November, 1936, when the price of gold touched

142/6d per ounce, and the end of June, 1937, when it was about 140/6d., the Fund is estimated to have lost about £6 Mn. With the gold holdings of the Bank and the Fund at their present level, it is estimated that every 3d. fall in the price of gold costs the Fund about £1 Mn., in addition to a reduction of about £350,000 in the market value of the Bank of England's original gold holding of about 28 Mn. ounces. A fall in the price of gold below about 131/- per ounce would therefore wipe out the whole of the Fund's profit, while at 120/- the Fund's loss would be approximately equal to the remaining profit on the Bank's gold. At any price below 120/- a net loss would be shown on the two combined.

This calculation takes no account of the accumulated interest cost, which, as estimated above, is already equal to three-fifths or more of the profits. Even if the price of gold and the size of the Fund's gold stock remain unchanged, it is estimated that by the end of 1938, at latest, the accumulated interest charges will be equal to the profits. To cover the annual interest cost, the price of gold would have to rise by about 2/6d a year.

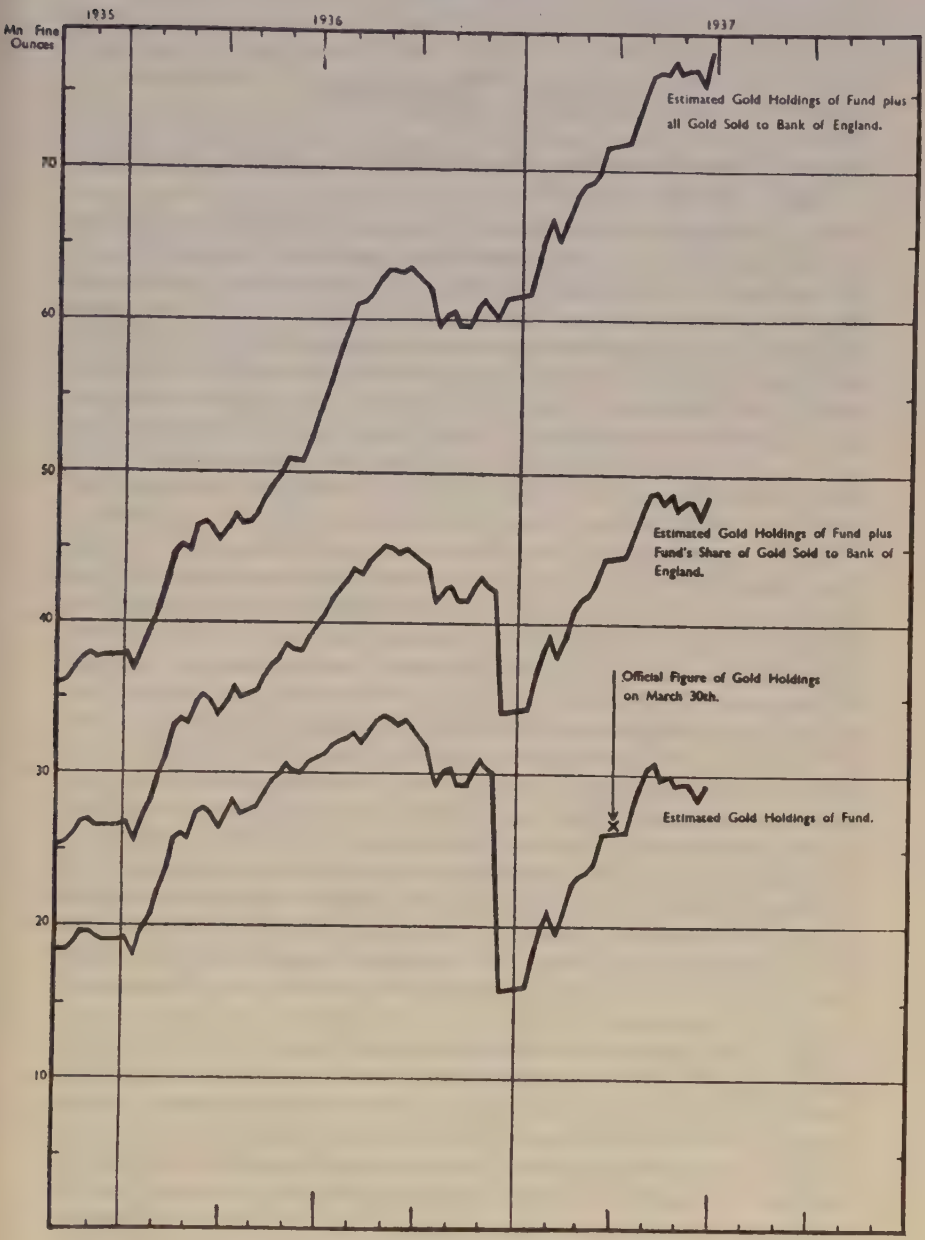
Note.—I am indebted to the Economic Research Division of the London School of Economics for assistance in making the calculations of which the results are embodied in this article.

ESTIMATED GOLD HOLDINGS OF THE EXCHANGE EQUALISATION FUND. TABLES.

Date.	Gold held by Fund	Gold held by Fund plus Fund's share of gold sold to Bank	Gold held by Fund plus all gold sold to Bank
1935.	Mn. Ozs.	Mn. Ozs.	Mn. Ozs.
Nov. 1	18·40	25·43	36·03
„ 8	18·44	25·57	36·31
„ 15	18·90	26·14	37·02
„ 22	19·61	26·85	37·73
„ 29	19·63	26·95	38·01
Dec. 6	19·14	26·52	37·72
„ 13	19·03	26·52	37·86
„ 20	19·03	26·52	37·86
„ 27	*	*	*
1936.			
Jan. 4	19·17	26·68	38·00
„ 11	18·18	25·67	37·01
„ 18	19·74	27·21	38·57
„ 25	20·74	28·21	39·57
Feb. 1	22·16	29·62	40·99
„ 8	23·87	31·33	42·70
„ 15	25·72	33·18	44·55
„ 22	26·06	33·64	45·12
„ 29	25·92	33·50	44·98
Mar. 7	27·34	34·92	46·40
„ 14	27·62	35·20	46·68
„ 21	27·36	34·94	46·42
„ 28	*	*	*
April 4	27·34	34·88	46·40
„ 11	28·34	35·88	47·40
„ 18	27·40	35·05	46·69
„ 25	27·54	35·19	46·83
May 2	27·72	35·46	47·25
„ 9	28·63	36·43	48·39
„ 16	29·49	37·27	49·25
„ 23	29·96	37·81	49·96
„ 30	30·72	38·62	50·96
June 6	30·33	38·27	50·81
„ 13	30·10	38·13	50·81
„ 20	30·84	39·11	52·25
„ 27	*	*	*
July 4	31·37	40·59	55·13
„ 11	31·92	41·60	56·86
„ 18	32·18	42·30	58·30
„ 25	32·31	42·88	59·60
Aug. 1	32·81	43·75	61·04
„ 8	32·24	43·42	61·18
„ 15	32·96	44·14	61·90
„ 22	33·59	44·83	62·76
„ 29	33·92	45·24	63·33
1936.	Mn. Ozs.	Mn. Ozs.	Mn. Ozs.
Sept. 5	33·68	45·09	63·33
„ 12	33·31	44·74	63·19
„ 19	33·60	45·00	63·48
„ 26	*	*	*
Oct. 3	*	*	*
„ 10	31·74	43·88	62·09
„ 17	29·49	41·66	59·84
„ 24	30·19	42·38	60·54
„ 31	30·33	42·54	60·68
Nov. 7	29·35	41·56	59·70
„ 14	29·35	41·56	59·70
„ 21	30·33	42·54	60·68
„ 28	31·03	43·21	61·38
Dec. 5	30·43	42·56	60·78
„ 12	30·05	42·22	60·40
„ 19	15·89	34·12	61·54
„ 26	*	*	*
1937.			
Jan. 2	*	*	*
„ 9	16·17	34·39	61·82
„ 16	18·43	36·67	64·08
„ 23	19·98	38·25	65·63
„ 30	21·11	39·41	66·76
Feb. 6	19·70	38·02	65·35
„ 13	20·97	39·32	66·62
„ 20	22·66	40·99	68·31
„ 27	23·36	41·71	69·01
Mar. 6	23·64	42·03	69·29
“ 13	24·20	42·60	69·85
„ 20	26·04	44·43	71·69
„ 27	*	*	*
„ 30	26·67	OFFICIAL	FIGURE
April 3	*	*	*
„ 10	26·31	44·58	71·96
„ 17	27·72	45·93	73·37
„ 24	29·14	47·25	74·79
May 1	30·56	48·59	76·21
8	30·84	48·90	76·49
„ 15	29·67	48·17	76·49
„ 22	29·91	48·59	77·20
„ 29	29·20	47·88	76·49
June 5	29·34	48·02	76·63
„ 12	29·34	48·02	76·63
„ 19	28·49	47·17	75·78
„ 26	29·27	48·44	77·77

* Note.—When figures for Treasury Bills are published for the last day of the quarter, the figures for the Saturday before, or the Saturday after, or sometimes both, are not published. The figures for the end of the quarter, when they are mid-week figures, cannot be used, for they are not comparable with the calculated total of bills issued by tender, which are end-week figures.

ESTIMATED GOLD HOLDINGS OF THE EXCHANGE EQUALISATION FUND.
CHART.



Book Reviews

National Income and Outlay. By COLIN CLARK. London: Macmillan. 1937. 304 pp. 12s. 6d.

This is a sequel to *The National Income, 1924–1931*, and follows to a considerable extent the same lines, with the incorporation of the additional subjects which Mr. Clark has treated in Journals and Memoranda during the past few years, all brought up to the most recent date. It is a mine of information over a wide range of subjects, and shows the great development in recent years of official statistics relating to production, employment, prices and cognate subjects. Mr. Clark has endeavoured to work these into a coherent whole, and though the results and methods may be subject to criticism, there can be nothing but praise for the patience and industry with which the problems have been attacked and the data exploited.

When a mathematical student encounters a new proof he will, if conscientious, first set out the problem in his own mind, and then work through the lines of the proof as independently as possible, carefully noting the hypotheses introduced. In reading statistics the same process should be followed; but whereas in ordinary mathematics the data are rigid and any numbers exact, in the kind of statistics before us the data are elastic, unprecise and the subject of judgment. Before accepting the conclusions of this or of any similar book, the student should master the definitions, trace the data to their source, find how they fit into the tables, and above all, separate that which is ascertained under definite rules of enumeration from that which is the subject of estimate from incomplete or collateral sources. Though Mr. Clark tells us the origin of his estimates and at least sketches his way of handling them, the reader will find great difficulty in following the numerical arguments and tabular mechanism; but since each of the problems handled is of immediate practical importance the effort is worth while.

The establishment of an estimate of the National Income resembles the fitting of a jig-saw puzzle or of a mosaic in a Roman site. But some of the pieces are missing, many are badly cut, some overlap, some come from a different puzzle or mosaic, some are elastic and subject to rapid changes with temperature or moisture, while the outline is irregular, and the boundary arbitrary. In the end we hardly know whether we have Phaeton with his chariot or Neptune with his Tritons. Thus we have innumerable estimates of what appears to be the same entity.

Mr. Clark has introduced a new definition of National Income with quite insufficient justificatory explanation. He adds the receipts from rates and indirect taxation to the total of personal and corporate incomes and the other elements which have hitherto been included. The total is an aggregate of the incomes of persons and the incomes of governments. Since the latter are spent in wages and salaries, which are already included, and produce no additional or uncounted services, it is hard to see what justification there is for calling the total National Income, a term already susceptible of many meanings, whatever may be the utility of the sum for some further calculations. The author himself appears not to have escaped confusion, for on page 139, under the heading redistribution, he states the amount (£942Mn) collected in taxation, direct or indirect, in 1929–30 as 21.5 per cent. of income which includes £515Mn indirect taxes. Another very doubtful item, included also in the earlier book, is the writing down of the value of traders' stocks when prices are falling, believed not to be allowed for in returns for income assessments ; this amount is a matter of conjecture, and it is at least open to question whether it is not better included on the side of capital.

It would be impossible to deal with all the subjects that are treated, and only a few can be taken in illustration. The fluctuations of the number of wage-earners (Table 28)[1] are based on the numbers of insured and employed, together with allowance for uninsured classes, and subtraction of a doubtful estimate of insured salaried persons. Now under the vagaries of insurance administration there have been sudden increases and withdrawals from the scheme. In particular from 1929 to 1931 the number insured increased 5.7 per cent., showing that many were drawn in from without, since the population of working ages only increased 1.5 per cent. in the two years. Hence the reduction in the wage-bill (reduction because of increased unemployment) should be greater than that shown in the Table. It is suggested that it is safer to work from the number of employable persons from the Occupation Census than to depend on the insurance figures. Table 28 is not, however, intelligible. The final column appears to be worked with the last multiplier given, itself wrongly headed, while the text would lead us to expect the use of the penultimate multiplier. This is a case where the student should check and understand every step before using the result.

In Table 55 the first part claims to be equivalent to a Table in the present reviewer's *Distribution of the Product of Industry*. Many of the entries are recognisable from Table IV of that work. But all 'employers and independent workers,' which include the itinerant knife-grinder and the Harley Street doctor, have been entered under Rent, Interest and Profits, and the total for Agriculture has been altered.

In Table 56 'profits,' in the penultimate column, includes rent and interest. It is somewhat misleading when considering the relation between 'profits' and wages in this Table not to allude to the very varying amount of capital per operative which is one explanation of the variation in the proportion. There is an unfortunate misprint of 156 for 15.6 for the percentage of profits to wages in mining, and the last entry in the Table should be 28 instead of 71.

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[1] In Table 37 the cross-references are wrongly numbered.

The differences between Mr. Clark's estimate for 1924, which is only modified here to agree with his new definition, and that by Sir J. Stamp and the present writer were analysed in ECONOMICA, 1933, pp. 138–142. The essential element of discrepancy lay in the relation between the number of wage-earners and the number of persons counted as occupied in the Population Census. Material for reconsidering the question in the light of the 1931 Census is given here in Appendix I. It has already been pointed out to Mr. Clark that whichever was the better estimate in 1924, for comparative purposes with 1911 it was necessary to use our account, not his. Nevertheless in Table 39 he compares his estimates for 1924 and subsequent years with what is said to be the earlier estimate for 1911. 'Is said to be' because not one of the numbers and hardly any word is the same in the quotation and the original. The adjustments are not important, but it is misleading to compare the percentage of total income that is classed as wages in 1911 with the percentage on a different basis in 1924. In *The National Income, 1924*, we found an increase in the proportion of wages to 'social income' from 43 to 44 per cent.; Mr. Clark finds a greater increase of wages (excluding shop-assistants) to 'Home-produced income less Government income,' viz., 39.5 to 42.1 per cent. Unfortunately the earlier publication is out of print, but it is suggested that in other cases where the originals are more easily accessible they should be collated with Mr. Clark's rearrangements of them.

There is a curious example in Chapter VI of the tenacity of estimates which have no real basis. If one turns to the Reports of the Colwyn Committee[1] that lead to the estimate (adjusted by Mr. Clark) that the average annual expenditure on drink was £10 13s. 6d. (notice the pence) by a standard working-class family, it will be found that there was no evidence offered that any working man ever drank, and that the figures depend on a former not very well-informed guess. This is one of the elements that lead to the statement on p. 148 "The net effect of taxation and local rates in 1935 can be described as a redistribution of £91 millions from the rich to the poor in the form of services, other than those provided from the proceeds of working-class taxation." Both the Committee and Mr. Clark have been too anxious to get a measurement to allow them to criticise their evidence.

Perhaps more than enough has been said by way of caution in accepting the results. It is more grateful to end with an expression of appreciation of the acumen with which the author has opened up new lines of investigation and of the stimulus which his work gives to all students of the problems of realistic economics that are now outstanding.

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[1] Compare the treatment of the same statistics in *New Survey of London Life and Labour*, vol. ix, pp. 263-4, by Mr. Nicholson.

A. L. BOWLEY.

Moscow in the Making. By SIR E. D. M. SIMON, LADY SIMON, W. A. ROBSON and J. JEWKES. Longmans, Green and Co. 1937. xii + 253 pp. With frontispiece and maps. 7s. 6d.

In 1854 Mr. Tracy Turnerelli published two volumes on Kazan. Dickens wrote in *Household Words:* "He is a happy man who has a mission; and we envy Mr. Tracy Turnerelli, whose mission is to be M.C. or Master of Ceremonies to the town of Kazan". Turnerelli had lived fifteen years in Russia; the present investigators of Moscow not more than four weeks. What is their mission? They are all four experts in various aspects of local government in their own country. One of them is a town planner. Like the Paris Haussmann, town planners will always be pleased with dictators. Their enemy is lethargy. The mission of the four English experts seems to be to stimulate lethargical English capitalism, just as Laski's book on justice in Soviet Russia was intended to "awaken" the legal profession. The occasional eulogy of Soviet conditions or principles is not to be understood as pro-Soviet propaganda, as it well may be at first sight. It is pure tactics. Bad tactics for "grown ups", because none of the experts would think for a moment that the English should exchange a bed of feathers for a bed of planks.

W. A. Robson has been patient enough to give a detailed account of the City Government of Moscow. As the author of a book on Local Government Audit, Mr. Robson has detected the resourcefulness of the one-party rule in manipulating the broad democratic appearances, the one political—but unsavoury—post-war invention of importance (138). "This repressive side of the system is immensely important" (67). On the whole he is generous, perhaps too generous.

J. Jewkes, writing on Industry and Finance, arrives at the conclusion that Mossoviet expenditure is rigidly prescribed by decisions of the state (89). Of the two questions: whether the people like it, and whether they could do better without, only the latter is implicitly answered in the negative; yet the face of democracy is somehow being saved. The word autonomy, when it occurs, seems, however, a misnomer. The surprise at the range of municipal enterprises, which would be considered in England *ultra vires*, is excessive. It shows that continental local government and Russian municipal government before the revolution are *terra incognita*. (See: M. Miller, *Economic Development of Russia, 1905–1914*, p. 144.)

Lady Simon, writing on education, is almost swept off her feet by a wave of admiration over the vastness of the educational programme, so often advertised already, yet she ends her chapter with the words: It is magnificent—but is it, after all, education? (142). The interlude about "bourgeois pedology" (129–137) is illuminating, and the only entertaining passage in the book. Lady Simon, like the other contributors, could not spare the time to turn to some of the special books on her subject published in English, by N. Hans, S. Hessen, and in the Russian Series of the Economic and Social History of the World War. Simple facts like the rate of numerical increase of schools and pupils during the last 50 years would have been adjusted.

Housing and The Building Trade, both by Sir E. D. Simon, are interesting, but reveal the difficulty of a comparative treatment. Prague and Warsaw since the war, not Manchester, are tackling difficulties similar to those of Moscow. Have these non-socialist cities done worse or better? At any rate Warsaw has also pulled down a huge cathedral on national grounds. As to Manchester, its houses would not do for Moscow. An exchange would mean disaster for the Muscovites. All the piping would burst in the first winter, as it would in Toronto, in spite of the gas fires recommended by Mr. Robson.

Wonderful things could be done in London by a power unhampered by private ownership and contractual rights. The endless rows of uniform houses occupied by the numerous "few" could be re-allotted. The obsolete coal cellars under the pavement could be transformed into galleries for the various pipes, rendered thereby accessible for repairs, etc., etc. Hence the eagerness of the authors to show the English public what can be attempted by an unlimited socialist authority becomes intelligible. That the authors do not see eye to eye with regard to "prestige" buildings (e.g., the Metro and the Soviet Palace) would be dangerous if they were Soviet citizens. To argue that there is a similarity between the attachment of the mediæval people to their costly cathedrals and the love of the Russians for the Moscow Underground would be treated as a deviation from the obligatory economic materialism, the only permissible interpretation of reality. Fortunately the authors of *Moscow in the Making* can disagree with impunity, and can admire without devotion. A devotee must be ready to sacrifice not only his leisure but, economically speaking, his belongings. In pre-war days the City could place in London £18 million of Russian municipal loans. The authors would hardly be prepared to recommend similar operations to-day. Yet they have a kind of confidence which may be called philosophical. They seem to believe the Moscow doctrine, not its practice, to be immune from the most hateful errors. As regards the respect for private rights and individual liberty they may be right. These prejudices have been thoroughly eradicated. But it would be rash to sell the birthright of English citizenship to provide a treat for town planners.

A. MEYENDORFF.

Kapitalbildung. By J. **Marschak** and W. **Lederer**. William Hodge and Co., Limited. 1936. ix + 315 pp. 10s. 6d.

Habent sua fata libelli. This book had been prepared several years ago when the two authors still worked in Germany, but the same reasons which brought their academic activity in that country to an end were also responsible for the long delay with which the present book is being published now and for the great difficulties which the authors had to encounter in order to complete the work in spite of the fact that the one is now working in England, the other in the United States. The fact that the book has been written by two German authors in exile and separated from each other, that it has been printed in Germany, published by an English firm and patronised by the Rockefeller Foundation unveils its somewhat dramatic history. At the same time, it accounts for a number of peculiarities of the book which must be understood by a fair-minded critic as the outcome of its peculiar history. It is easy to see that the co-operation of the two authors—always a somewhat hazardous procedure—shows a certain lack of co-ordination, that the book does not form that coherent whole which, under different circumstances, might have been expected, and that in certain respects it is somewhat lagging behind the latest developments in the underlying theories. In the same vein, one might object that the theoretical part written by Dr. Marschak seems unduly concise and difficult in style while its relation to the second statistical part, written by Dr. Lederer, is implicit rather than explicit, and if one were pedantic enough one might take exception to the many and not always harmless misprints. But once all this has been said one should look at the other side and welcome this book as the happy result of the perseverance and the intellectual keenness of the authors and of the generosity of their sponsors.

The first theoretical part, written by Dr. Marschak, is called "Hauptgrössen der Kapitalwirtschaft". Much influenced, as the author states, by Böhm-Bawerk, Wicksell, Irving Fisher, Lindahl and Myrdal, it gives a systematic-terminological treatment rather than a causal theory of the capital market. This is a limitation of its scope, not of its usefulness. Everybody will agree, indeed, that the use of the terms most important in this field—"saving", "investment", "income", etc.—has become so muddled that Dr. Marschak's valiant attempt at clarification must be welcomed. It goes without saying, however, that there are many points inviting discussion, so e.g. his treatment of the question of the necessary equality between savings and investments (p. 32) where the salient point seems to have been missed by identifying savings with the actual supply of liquid means and ignoring savings in the form of idle balances. It is here as at some other points that one has the feeling that what the author says does not represent his most recent views on the matter. A particularly interesting section (pp. 42–51) deals with the factors determining the supply of "capital", especially with the "interest sensitivity" of saving (cf. for an improved statement also J. Marschak, on "Measurements on the Capital Market", *Proceedings of the Manchester Statistical Society*, 1936). Here I miss some points with which I have dealt in my paper "Der Einfluss sozialwirtschaftlicher Faktoren (Einkommenshöhe und Einkommenschichtung) auf die Kapitalbildung".[1] I also wonder whether Dr. Marschak would have use for the concept of the "quality" of saving with which I have tried to work on an earlier occasion and which could be sub-divided into "time-quality" (=degree of liquidity preference) and "quality in use".[2]

As for the second (statistical) part, "Umfang und Formen der Kapitalbildung in wichtigen Ländern," written by Dr. Lederer, it is unfortunate that it is very difficult to do real justice to it in a review like this by giving it the space which it deserves. It raises points which could be discussed endlessly, but one gets the impression that Dr. Lederer would have a very firm stand in such a discussion. His care and circumspection are as admirable as his zeal and his gift of combination, and even where his statistical explorations seem bold he wins confidence by his ingenuity. It seems safe to predict that this part of the book will become an outstanding source of reference.

WILHELM RÖPKE.

The Development of the Business Corporation in England, 1800–1867. By BISHOP CARLETON HUNT. Harvard Economic Studies No. 52. Harvard University Press and Humphrey Milford, Oxford University Press. 1936. xii + 182 pp. English price 10s. 6d.

British Corporation Finance, 1775–1850. A Study of Preference Shares. By GEORGE HEBERTON EVANS. Baltimore: The John Hopkins Press and London: Humphrey Milford, Oxford University Press. 1936. 208 pp. English price 10s. 6d.

The principle of complete freedom to conduct business enterprise under the company form of organisation, with the associated attributes of limited liability and transferability of shares, has not been seriously challenged since the middle of the nineteenth century. The practice has been subject to continual criticism and review but the doctrine then established that freedom of contract made this form of association a right and not a privilege remained unquestioned. This doctrine was a reflection of a social and economic philosophy which cannot be better expressed than in the words of Sir Robert Lowe in his classic speech which swept the Joint Stock Companies Bill of 1856 on to the statute book. " Man, Sir, with his free will, his caprices and his errors is as much under the rule and government of a natural law as the planet in its orbit or as water which always seeks its own level. Those laws planned by Infinite sagacity have the power of correcting and compensating errors—one extreme invariably producing another—dearness producing cheapness and cheapness dearness; and thus the great machine of society is constantly kept oscillating to its centre . . . Who could have imagined it possible that a state of society resting on the most unlimited and unfettered liberty of action would tend more to the prosperity and happiness of man than the most matured decrees of senates and States ? These are the wonders of the science of political economy and we should do well to profit by the lesson which that science has taught."

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[1] In *Kapitalbildung und Steuersystem*, Verhandlungen und Gutachten der Konferenz von Eilsen, Berlin, 1930, Vol. II, pp. 212–228.

[2] W. Röpke, "Die Quellen der deutschen Kapitalbildung 1908–1913 und 1924–1929," in *Kapital und Kapitalismus*, ed. by B. Harms, Berlin, 1931, Vol. I, pp. 289–307.

It is true that the opposition could also draw Providential aces out of its sleeve, but McCulloch was fighting a rearguard action against limited liability when he thundered " In the scheme laid down by Providence for the government of the world there is no shifting or narrowing of responsibilities, every man being personally answerable for all his actions. But the advocates of limited responsibility proclaim in their superior wisdom that the scheme of Providence may be advantageously modified."

Those were the days ! But the post-war oscillation of the great machine of society about its centre has tended to replace, or at least supplement, Providence by Exchange Equalisation Funds, and the higher criticism has even profaned with its touch the holding company. In the U.S.A. an attempt to protect the small business against the competition of the large corporation has revived the notion that joint stock enterprise is a mere creation of the law, a man-made facility which can and should be modified in accordance with social needs.[1] Even in this country Mr. Keynes, with the approval of Professor Pigou, has queried the desirability of permitting free transferability of shares.

All this gives an added fascination to a chapter in economic history which has been somewhat neglected, and the two books noted above are a valuable and timely contribution to the subject. Dr. Hunt's treatise is a worthy addition to the Harvard series and as a succinct documented record could hardly be bettered. There is a really excellent bibliography. Of Professor Evans' work it need only be asked where else one can find a studious account of the origin of the preference share. He is certainly committed to a sequel, and the 1s. deferred share awaits his research. Both works are a happy result of research grants, and American scholarship has filled a gap left open by British economists.

G. L. SCHWARTZ.

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[1] See dissenting judgment of Mr. Justice Brandeis in Leggett Co. v. Lee, March, 1933.

Southern India: Its Political and Economic Problems. By GILBERT SLATER. Allen & Unwin. 1936. 375 pp. 15s.

In this volume Dr. Gilbert Slater gives a most interesting account of his experiences and observations, mainly, but not entirely, in Southern India, from 1915 to 1922. The period was one of outstanding importance. It covered the years of post-War economic adjustment, including the period of wild fluctuations in the rupee sterling exchange, the inauguration of the Montagu-Chelmsford Reforms, the beginnings of severe labour unrest and of modern trade unionism in India, and the rise of the Home Rule movement. Madras was in several aspects the most interesting province during these years. It was, for instance, the scene of the origin of modern trade unionism, and it proved to be the province in which Dyarchy was worked in the manner probably most nearly approximating to that envisaged and desired by the authors of the reforms of 1919.

Dr. Slater, first as Professor of Economics, later as Acting Publicity Officer to the Government of Madras, and as a nominated member of the Madras Legislative Council under the reforms, made the most of his exceptional opportunities. In his Preface he points out how faint is the line of demarcation between economic and political science everywhere, but especially in India, and his broad sociological approach to the manifold problems of Indian life is certainly one of his most striking merits.

The book is, indeed, a mine of information on the most diverse subjects—geographical, agricultural, historical, biographical, mythological, anthropological, religious, institutional and even linguistic. At one moment we visit with him some remote rural area, and study the daily lives and customs of the villagers. At another we visit the up-to-date works of the Tata Iron and Steel Company, and he discourses upon the origin and functions of the Managing Agent System. At another the difficulties of the Tamil language are explained; whilst from time to time pertinent comments are passed upon such subjects as Public Health, debt slavery and the reasons for the prevalence of an "inverted colour sense." This latter, by the way, is in direct opposition to Professor Radhakrishnan's story of the Creation, according to which God underbaked the first "lot" of men (the whites), overbaked the next "lot" (the negroes), but turned out the last "lot" just right: *i.e.*, brown!

Whatever the subject under discussion, the author reveals his characteristic merits: first, his breadth of view, to which attention has already been drawn; second, his insistence upon first-hand, detailed observation; third, his remarkable power of combining sympathy with candour. Dr. Slater is never satisfied with second-hand facts or opinions, however widespread their acceptance. He must exercise his own powers of critical observation and judgment. Contrary to what is sometimes believed, candid but sympathetic criticism is highly appreciated in India, and this—together with the author's method of teaching, namely: by stating often well-known facts in an original or striking fashion, so as to bring home their full implications—must have rendered him an invaluable professor in Madras. It is true that he is more interested in the practical application of commonsense economics than in the weaving of abstract or novel theories, but at times, when commonsense is at a discount (in other countries, as well as in India!) this limitation may in some respects be considered as a virtue. It is, at any rate, certain that those who do not know India will find in these pages a wise and sometimes witty guide to a surprisingly large number of the social, political and economic problems of that puzzling, heterogeneous and fascinating land. Those who do know India must be learned and penetrating indeed if they, as well as learning many new facts, fail to obtain new light on many problems.

Three minor errors may be noted for future editions. In the Foreword Lord Willingdon refers erroneously to the author's "twenty" years in India. On p. 223 over-compression leads to a somewhat misleading account of the course of prices and hence of the causes of the subsequent currency crisis. On p. 314 it is asserted that Provincial Franchise was extended to women in Madras and Bombay only. Actually, in the words of the Simon Commission (Vol. I, p. 134) "the Electoral Rules did not establish women's suffrage, but they empowered the Legislative Councils by resolution to remove the sex barrier themselves, and this has now been done in every province."

VERA ANSTEY.

The Cacao Industry of Trinidad. By C. Y. SHEPHARD. Government Printing Office, Trinidad. 1936. Series II, 3s. 0d.; Series III and IV, 4s. 6d.

The Trinidad Cacao Industry has been declining in output and relative importance since 1921. In particular, whereas formerly yields per acre were as high in Trinidad as those of British and French West Africa, which with Brazil now form the chief sources of supply, to-day they are only about half as great as those of the Gold Coast. The two volumes to be reviewed attempt to explain this state of affairs, and provide a continuation of Series I, which gave a history of the industry from its origin up to 1932. They present the results of Mr. Shephard's detailed researches into the factors affecting profits, and the means (if any) by which the industry may be rehabilitated. The first volume consists mainly of an exhaustive analysis of expenditure, revenue, yields and profits (or losses) on a large number of estates, and gives data which enable the individual planter to judge whether or not his results compare favourably with those of others. The study reveals the fact that there is no definite or constant correlation between, on the one hand, such factors as yield, expenditure or cultivation, use of manures, size or age of holdings, and the degree of crop specialisation, and—on the other hand—profits or losses. In the second volume the author proceeds to co-ordinate the results of scientific specialists, of experienced planters, and of economic analysis into an integrated whole. This provides a means of discovering the fundamental causes of profit or loss, and a basis for practical recommendations. For instance, the analysis of the relation between age of the field and yield shows which factors associated with age depress the yield, and the extent to which such factors are remediable. The study definitely proves that a mere return to traditional methods of cultivation which were abandoned during the depression would not rehabilitate the industry.

The method pursued, combining as it does theoretical analysis with realism, is undoubtedly fruitful and illuminating, and might well be applied to the study of other crops.

VERA ANSTEY.

Berechnung und Ausschaltung von Saisonschwankungen. By A. WALD. Austrian Institute for Trade Cycle Research. Vienna: Julius Springer. 1936. viii + 140 pp. RM. 7.80.

This excellent monograph, in exposition lucid, in substance both systematic and ingenious, makes two welcome contributions to the equipment of the statistical economist: it develops a general theoretical treatment of the seasonal element in time-series, and it puts forward a new and attractive method of separating that element.

Each of the methods now commonly used has its intuitive appeal; but this fact in itself should make us conscious of the need for a general treatment of the results to which different methods lead and the relations which these methods imply between the seasonal and the other components. This need Dr. Wald meets. In his second chapter he examines the principal methods now used, and shows, for instance, that "if in the link-relative method or the method of relatives to trend we use either the arithmetic or the geometric mean, the results we get are the same as those which follow by a simple correction for trend from the values given by the monthly-average method when the same kind of mean is used" (p. 24; my translation is free). The third chapter examines in the same way the application of the variate difference method to remove first of all the random element. The treatment here demands higher mathematical attainments than are called for elsewhere, and I have not been able to follow it closely. I hope that more expert readers will not fail to test an argument which leads to an arresting conclusion: namely that the presence of a seasonal pattern in a series will itself bring about that stability of the mean squared differences which in the variate difference method is the criterion of the isolation of the random element, and that if this method is applied to a seasonally-patterned series, the supposedly random element which it isolates will in fact itself show a seasonal pattern.

Dr. Wald's own method is designed to overcome the difficulty that, when the intensity of seasonal movements changes, a single index applied to the whole run of a series will yield a "corrected series" which itself shows a seasonal pattern, negatively correlated with the original. Dr. Wald provides a moving correction, as follows. We first calculate for each month the average of the differences ψ between the original entries and the centred twelve months' moving average, and adjusting these twelve averages so as to bring their sum to zero we obtain corrected monthly averages, $\alpha'_1 \ldots \alpha'_{12}$. To find the seasonal element in a given entry, say September 1932, we then multiply α'_9 by a factor μ determined so as to minimise the sum of the squares of the differences between ψ and $\mu\alpha'$ from April 1932 through March 1933. This brief description, however, touches only on the apex of a pyramid of argument, which shows upon what assumptions the method may be proved to isolate a seasonal movement of gradually changing intensity, which illustrates it by application to some Austrian series, and which examines the adjustments necessary when the "corrected series" proves to retain some traces of seasonal regularities.

E. H. Phelps Brown.

The Theory of Gold Supply with Special Reference to the Problems of the Witwatersrand. By W. J. Busschau. Oxford University Press. 1936. x + 194 pp. 10s. 6d.

In this volume, the first to appear in the series of "Oxford Studies of Economics," Dr. Busschau makes a very interesting essay in the application of the methods of marginal analysis to the practical problems of the Witwatersrand gold mines. After a preliminary discussion of the methods and difficulties of valuing and amortising speculative and wasting assets, he proceeds to his main investigation of gold-mining costs and yields. Expressing these in the form of average and marginal cost and revenue curves (together with a curve for "working costs," the exact relation of which to the marginal cost curve is not made altogether clear) he shows how to determine the most profitable rate of output for a mine with given equipment and a given tonnage of a given grade of ore. He next shows how to determine the grade of ore which it will pay to work, and finally, and with less detail, how to determine the best scale for the equipment of a mine, and therefore its expected life.

On one point the discussion is disappointing. One of the most interesting questions about the economics of gold-mining is the order in which it will pay best to work ore of different grades. The practice of the Rand mines (perhaps determined by those whose interests lie in the maximisation rather of the life of the mines than of the return to shareholders) has been to mine high and low grade ore together so that the average grade of ore milled in any one year approximates to the average grade of the ore reserves. This practice leads to the rather anomalous result that a rise in the price of gold, by lowering the minimum grade which it pays to mine and the average grade of the ore reserves, also lowers the average grade actually milled ; consequently, until the tonnage of ore milled increases sufficiently to offset the lowering of the grade, the quantity of gold produced from existing mines falls as a result of a rise in its price.

On this point, Dr. Busschau of course appreciates that with positive rates of interest it is *prima facie* more profitable to mine the rich ore first, but replies that this is true only if there is no change in other conditions. " If the price of one factor (*e.g.*, wages), rises, much of the lower grade would later become unprofitable. In that case there is the danger that the volume of ore discarded is so great that its later removal from the profitable class may actually diminish the present value " (p. 93). But this is a mistake, which Dr. Busschau would surely never have made if he had given as much thought to this question as to the other parts of his book. His argument leads the other way. If there is to be a rise in costs, independent of the order in which the ore is worked, the simplest numerical example will show that the more high grade ore which is worked before the rise in costs and the more low-grade ore which is therefore left to be abandoned as unprofitable after the rise in costs, the greater is the profit earned over the whole life of the mine, even at a zero rate of interest. The important point is not the chance of an independent rise in costs, but the probability or certainty of a rise in costs directly due to the policy of mining the richer ore first. It is a pity that Dr. Busschau did not give more careful attention to this point, for it would have been interesting to see him use his technique to determine the degree of priority which it would pay to give to high-grade ore under different assumptions regarding interest rates and costs.

It is the more surprising that Dr. Busschau missed this point since his treatment of the closely related problem of the effects of taxation is most useful. Among other things, he shows conclusively that, by adjusting taxation so as to induce the mines to mill ore of a lower grade than would otherwise have proved profitable, the government is in fact subsidising the production of the gold contained therein at a loss to the community. The profit to the mines and the government together is less than if this ultra-marginal ore had been left in the ground, though since more than the whole of the loss is borne by the government, it pays the mines themselves to mill it. Dr. Busschau suggests changes in the tax formula which, while securing the general purposes which the South African government appears to have in mind, would avoid this defect in the present system.

A good deal of this book will probably be too difficult for most of those who are not already familiar with the technique used, though even to these the descriptive passages will be useful. But to students of mining who are familiar with economic technique, or to students of economics who are either interested in mining for its own sake or who will enjoy seeing a generally elegant application of economic analysis to practical problems, it may be strongly recommended.

F. W. Paish.

Interest and Prices. A Study of the Causes Regulating the Value of Money. By Knut Wicksell. Translated from the German by R. F. Kahn with an introduction by Professor Bertil Ohlin. London. Macmillan & Co. Ltd. 1936. xxxi + 219 pp. 12s. 6d.

Nearly forty years lie between the publication of *Geldzins und Güterpreise* and the appearance of its English translation. During this interval parts of the doctrine of the book have been discarded not only by Wicksell's critics but by the author himself. This is hardly surprising; for the book represented an attempt to break new ground rather than an exposition of a well-established theory.

What determines the general level of money prices? This is the question from which Wicksell started out. A review of previous attempts to tackle the problem brought him to the conclusion that the Quantity Theory, while open to serious criticism, constituted the only theory in existence. In this theory the most question-begging element is the velocity of circulation of money. It was while speculating upon velocity of circulation, one of "the flimsiest and most intangible factors in the whole of economics" (p. 42) that Wicksell picked up the thread leading to his central doctrine. For it occurred to him to put the case of a pure credit economy,[1] in which "the quantity of money units" and hence also their "average velocity of circulation" disappear altogether. We have instead an infinite quantity of potential bank credit (each unit of which is assumed only to effect one exchange before returning to the bank); the quantity of this credit that actually enters into exchange against goods is determined by the rate the bank charges for credit, conditions of demand being given. Now this conclusion that, other things equal, the price level varies inversely with Bank rate is simply an application of the Quantity Theory. In Wicksell's pure credit economy MV is replaced by the total amount of credit borrowed from (and deposited with) the bank, which therefore "determines the exchange value of money." However, the connecting link which is here supposed to exist between bank rate and the amount of credit drawn is missing until we know what entrepreneurs *expect* the future price-course to be. Wicksell assumed they expect prices to remain at their current level, and in this way he showed that if prices are not *constant* there will be disequilibrium. The general case, of which the above is a special example, is that if the actual price-course deviates from its expected path, there is disequilibrium. Any rate of interest charged by the bank is consistent with equilibrium, in the sense that entrepreneurs have no inducement to alter their activities, provided we suppose that the price level changes in such a way as to reduce the nominal rate of interest to the rate of return on the existing enterprises. Conversely, any change in the price level is consistent with equilibrium provided we assume that the nominal rate of interest is so adjusted to the price-course as to represent the rate of return on the existing enterprises. What position would actually be one of equilibrium is indeterminate until we know the velocity of circulation of money, or the extent to which people hoard.

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[1] In the course of the exposition of this case an error in the translation occurs. On pages 73–74 the German clause "welcher dem von ihr selbst geforderten nur wenig nachsteht" has been rendered as "bearing little relation to the rate which it itself demands" instead of "falling little short of the rate which it itself demands." The whole sentence should read: "The Bank must of course pay interest, at any rate on long term deposits (the rate falling little short of the rate which it itself demands)."

It appears therefore that it was from thinking too closely in terms of the Quantity Theory that Wicksell was led to believe constant prices to be a necessary condition of equilibrium on the capital market. Although it is now fairly widely agreed that this conclusion rests on special assumptions about price-expectations and has no general validity,[1] the usefulness of Wicksell's inquiry is surprisingly little diminished by the withdrawal of what looks like a central plank from his platform. For while he thought he was establishing a proposition about interest rates and the general price level, in fact he was exposing the mechanism by which interest rates react on the relative prices of certain groups of goods (which we may shortly, if inaccurately, describe as consumption goods and production goods). Running along through the main chapters of the book, somewhat below the surface of the argument, we find an analysis of the dynamic process by which adjustments in the rate of interest shift productive activity from one productive method to another. It must, however, be admitted that Wicksell underrated the scope of variations in total activity accompanying variations in relative activity in different types of industry. He explicitly denied the possibility of substantial changes in total employment and total output (p. 143). Once again, he was led to do this by his preoccupation with the general price level; for he wanted to get away from the possibility that a tendency towards rising prices might be counterbalanced by an increase in output. Both practical experience and a more detailed theoretical analysis of the effects of changes in the complex of interest rates upon production indicate, however, that a shift in productive activity towards more capitalistic processes of production will be accompanied by an increase in total activity, and vice versa.

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[1] Wicksell himself subsequently conceded that his case was less general than he had suggested in *Interest and Prices*. A discussion of the point may be found in Myrdal's article "Der Gleichgewichtsbegriff als Instrument der Geldtheoretischen Analyse," *Beiträge zur Geldtheorie*, edited by F. A. von Hayek.

In the discussion of interest it has long been felt that the " natural rate of interest " constituted a weak spot. Wicksell himself soon became dissatisfied with it. The fundamental mistake attaching to the introduction of this concept was that it involved an attempt to compare a monetary economy with a non-monetary economy. Wicksell rightly stated that his whole thesis of upward and downward cumulative processes was dependent upon the existence of a " normal " interest rate from which bank rate might deviate. But he was wrong to seek his norm in a non-monetary economy. Thus we have to substitute for the " natural rate " the " equilibrium rate, or rather complex of rates, in a monetary economy." A monetary economy would be in equilibrium if the complex of interest rates actually charged in the market corresponded to the complex of rates of (expected and actual) return to investment. A cumulative process—not necessarily in the sense of a continuous and rapid price-change, as witness the American boom of 1929, but in the sense of a continuous shift in the direction and amount of productive activity—would occur if the complex of rates ruling in the market deviate from those which are the equilibrium rates for the existing structure of production.

Interest and Prices contains the seeds of a large part of twentieth-century monetary analysis. In addition to the problem of dynamic changes in the structure of production, which has now received detailed treatment in Professor Hayek's work, Wicksell touched the central spring of all the recent theories dealing with the relationship between savings and investment. The early chapters of the book give clear statements of some of the most fashionable ideas about liquidity preference. After all the developments that have already been made, and all the modifications that have to be introduced, the work remains extraordinarily suggestive. It constitutes an excellent source for further studies in monetary theory.

Readers of *Interest and Prices* will appreciate Mr. Kahn's smooth style and careful translation. They will also profit by Professor Ohlin's helpful introduction, which guides us through the main developments in Wicksell's position, indicating those points in the doctrine of the book that were later substantially revised by the author. A translation of Wicksell's article " The Monetary Problem of the Scandinavian Countries " is a welcome addition to the main text and gives us some further indication of the views Wicksell held on monetary theory at the close of his life.

H. Makower.

Recent Important Foreign Books on Economics

The Librarian of the British Library of Political and Economic Science has made arrangements, as a supplementary check to the purchases of the Library, to receive regularly every quarter from competent foreign scholars lists of the most important books in their respective fields of study published in their own countries (or languages as the case may be) during the preceding three months. It appeared to the Editorial Board of ECONOMICA that these lists might prove useful to other institutions, and the lists of books on economics will therefore be published regularly in future issues of ECONOMICA. It must be understood, however, that the Editorial Board of ECONOMICA accepts no responsibility for the selection of the books included in the list.

AMERICAN BOOKS

(Selected by Professor H. Gideonse).

CLARK, J. M., AND OTHERS: *The National Recovery Administration: Report of the President's Committee of Industrial Analysis.* Washington: Committee of Industrial Analysis. x, 240 pp.

HART, A. G.: *How the National Income is Divided.* Chicago: University Press. Public Policy Pamphlet, No. 23. 28 pp. $-.25.

MEAD, MARGARET, *ed.*: *Co-operation and Competition Among Primitive Peoples.* New York: McGraw Hill. xii, 531 pp. $4.00.

MITCHELL, WESLEY CLAIR: *The Backward Art of Spending Money, and other Essays.* New York: McGraw Hill. vii, 421 pp. $3.00.

VINER, JACOB: *Studies in the Theory of International Trade.* New York: Harper. xvii, 650 pp. $4.50.

WHITTLESEY, CHARLES RAYMOND: *International Monetary Issues.* New York: McGraw Hill. ix, 252 pp. $2.50.

YOUNG, J. P.: *The United States Silver Policy.* New York: Foreign Policy Association, Foreign Policy Reports, Vol. xii, No. 8. 14 pp. $-.25.

GERMAN BOOKS.

BÖHM, F.: *Die Ordnung der Wirtschaft als geschichtliche Aufgabe und rechtsschöpferische Leistung.* Stuttgart: Kohlhammer. xxxi, 189 pp.

LUKAS, E.: *Aufgabe des Geldes.* Stuttgart: Kohlhammer. xix, 657 pp.

RÖPKE, W.: *Die Lehre von der Wirtschaft.* Vienna: Springer. vi, 195 pp.

(*In subsequent issues it is hoped also to include lists of French and Italian books.*)

Books Received

Alimenti, C.: *La questione petrolifera italiana.* Guilo Einaudi, Turin. 1937. 279 pp. 15 Lira.

Annali di Economia. Universita Commerciale Luigi Bocconi, Milan. 1937. 577 + 64 pp. 60 Lira.

Balas, C.: *The Necessity of Theoretical Points of View and of the Establishment of Theoretical Aims of Modern Demography.* Journal de la Société Hongroise de Statistique. 1937. 53 pp. No price stated.

Barone, E.: *Le Opere Economiche.* Nicola Zanichelli, Bologna. 1936. 533 pp. 40 Lira.

Baster, A. S. J.: *The Twilight of American Capitalism.* P. S. King & Son, Ltd. 1937. ix, 218 pp. 9s.

Baudin, L.: *Les Illusions du Crédit.* Albert Lévesque. Montreal. 1936. 317 pp. Fr. 1.

Benoy, K. S.: *The Sociology of Population.* N. M. Ray-Chowdhury & Co. Calcutta. 1936. viii, 139 pp. 3 rupees.

Bowley, M.: *Nassau Senior.* Allen & Unwin, Ltd. 1937. 358 pp. 15s.

Corporation Survey Committee of the Twentieth Century Fund: *How Profitable is Big Business?* Twentieth Century Fund Inc., New York. 1937. 201 pp. $2.00.

Dolezel, K.: *Gewerbepolitik und Industrie.* Verlag Rudolf M. Rohrer, Brünn. 1937. 67 pp. No price stated.

D'Souza, V. L.: *Economic Development of the Mysore State.* Bangalore Press. 1937. 49 pp. As. 8.

L'Ecole des Hautes Etudes Commerciales de Montreal: *Etudes Economiques.* Editions Beauchemin, Montreal. 1936. 558 pp. Fr. 5.

Elliott, S. R.: *England Cradle of Co-operation.* Faber & Faber, Ltd. 1937. 287 pp. 8s. 6d.

Einzig, P.: *World Finance 1935–1937.* Kegan Paul, Ltd. 1937. xvi, 342 pp. 12s. 6d.

Fong, H. D.: *The Growth and Decline of Rural Industrial Enterprise in North China. Industrial Capital in China. Industrial Organisation in China.* Bulletins No. 8, 9, 10. Nankai Institute of Economics, Nankai University. 1936, 1936 and 1937. 83, 68 and 88 pp. 75 c., 75 c., 75 c.

Facing the Tax Problem. Twentieth Century Fund Inc., New York. 1937. 606 pp. $3.00.

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Technical Improvements, Unemployment and Reduction of Working Hours

By A. G. B. Fisher

I

Introduction.

With unemployment a pressing problem in all parts of the world, it is scarcely a matter for surprise that an increasing amount of attention should have been directed in recent years to the possibility of removing or mitigating its difficulties by a reduction in the amount of time normally devoted to economic activity. Not only have left-wing political groups steadily pressed for the reduction of working hours as a remedy for unemployment, but the idea has also found considerable support in quarters which usually display a different political colour.

In these circumstances it is clearly a matter of some importance to examine critically the relationship which exists, or is alleged to exist, between a reduction in the length of the working day and a diminution in the volume of unemployment. On the one hand, incautious advocates of increased leisure have often been inclined to talk as if it were merely a matter of distributing more evenly the amount of employment which is available; the difficulties which arise when it is claimed that the man who works a shorter day should continue to receive the same income as before are ignored or glossed over, with perhaps the suggestion that a reduction in working hours will so much improve the efficiency of labour that as much as, or even more than before will be produced, and there will therefore be no excuse for any reduction of wages. There is much historical evidence to support the idea lying behind this suggestion, and there is no *a priori* reason for supposing that in every industry we have already arrived at exactly that length of working day which, from the standpoint of physical production, will give the best results. But clearly to the extent to which the plea is valid, a reduction of working hours does nothing to redistribute employment. Although working shorter hours, the men employed do as much effective work as before, and there is apparently nothing left over to be done by those at present out of work.

As against this possibly over-optimistic view, employers often tend to discuss the problem exclusively in terms of costs. If hours are reduced, without a corresponding reduction in pay, the costs of production will rise. In highly mechanised industries, costs may rise even if wages are correspondingly reduced, because, if the period of time during which machinery can be effectively used is diminished, the overhead charges per unit of output will tend to rise. At the same time, employers are, or ought to be, a little embarrassed by the appeal to history, which shows that similar objections to those which they are now raising have often been urged in the past, and shown by later experience to be without foundation.

II

Shorter Working Hours for Robinson Crusoe.

A simple appeal to history is, however, inconclusive unless at the same time some more detailed analysis is attempted of the factors which in real life have often invalidated the employers' claims. In the analysis which follows no reference will be made to the possibility of working double shifts. The scope for such action is in any event rather narrow, and it is not believed that the general conclusions suggested would require any important modification if the argument were complicated by reference to this subject.

It will be convenient to begin our survey by first examining the simple analogous case of an isolated worker, of Robinson Crusoe for example, before he met Man Friday. After a certain time, Crusoe may be presumed to have worked out for himself a reasonably efficient daily and annual schedule of toil, the performance of which will ensure him the modest standard of comfort which is all that an isolated worker can expect. To each task which he regards as important, he will allot a certain number of hours, while some leisure will also no doubt be available during which he can amuse himself or improve his mind without any thought of raising his standard of material welfare.

How should such an individual approach the question of reducing the number of his working hours? He may gradually become convinced that he has underestimated the value of leisure, but on the twofold assumption (*a*) that the allocation of time between different tasks is already such as to ensure the most efficient results, and (*b*) that no new factors begin to operate which are likely to increase the efficiency of his labour, he can enjoy greater leisure only by cutting down the volume of his productive output and thereby lowering his customary standard of living. It may be quite worth while to do this and if Crusoe deliberately makes his choice, nobody is entitled to reproach him. He must, however, clearly realise the intimate connection between his increased leisure and the diminished output of economic goods.

It is quite possible indeed that Crusoe might discover that his original schedule of work was not as efficient as he had imagined. By taking an additional half-hour off now and again and spending it in rest or in invigorating exercise or in reflection upon the technical problems of production, he might find that his efficiency during working hours had so much improved that he could enjoy as big an output as before, or perhaps even bigger, and at the same time have more opportunities for leisure. Crusoe would, however, then be faced with a new problem. It is highly improbable that the efficiency of his efforts would increase in precisely the same proportion in each of the activities in which he engaged, and even if it did, it is quite certain that he would not desire to increase his output at the same rate in each field of production. With the supplies which he was able to command of some things he might already be fairly satisfied, and he would accordingly be inclined to apply the more vigorous efforts, which his increased leisure now enabled him to exert, exclusively to other fields of activity, the fruits of which he had hitherto been able to enjoy only in limited quantities. In other words it would become necessary for him to elaborate another schedule of work, reducing the time allotted to certain tasks, though not in equal proportions, and increasing the time allowed for other kinds of work.

The third case and, in the world in which we live, the much more important case still remains. As a result of closer study of such technical books as he has been able to salvage from the wreck, or of the discovery of some unexpected stretch of fertile country, or of the cultivation of his own native inventive genius, Crusoe might find that, even in the absence of any lively desire for greater leisure, the technical efficiency of his work had so much increased that he was in a position, quite independently of the reactions of increased leisure upon the quality of his work, either to increase the volume and variety of his output or to maintain output at the old level and enjoy more leisure ; if he should think it more reasonable to compromise between these two positions he might simultaneously enjoy larger output and more leisure, though in neither case, of course, to the same degree as if his preference had been exclusively for one of the alternatives. Whatever his choice in these circumstances, Crusoe would again be faced with the problem of reorganising his schedule of work. He must decide how long he wished to work, and further he must reallocate his working time in proportions which might differ quite radically from those to which he had been accustomed. His working time would probably be most drastically reduced in those fields of activity which were most affected by the discoveries which he had fortunately been able to make or the products of which he felt were already abundantly supplied. Working time would be less diminished or might even be increased in fields where his ability to control his environment had not much increased, or where an increased production would afford him unusually keen satisfaction, and if the number of improved methods which he discovered was large the number of new satisfactions which he might enjoy would also be large. Crusoe might for some time be much embarrassed in working out correctly his new schedule. Where drastic revision of a customary scheme is necessary it is easy to make mistakes. He might fail to reduce sufficiently the amount of time he devoted to some process the products of which, in consequence of greater technical efficiency, had become available for him in greatly increased quantities. He would " over-produce " here, with a corresponding error of " under-production " somewhere else. But though this means that he could not enjoy as high a standard of living as he might have done, the general scheme of his life would not be seriously upset and by a process of trial and error he could hope with little delay to correct his errors and finally to allocate his time in the most efficient and satisfactory proportions.

III

Shorter Working Hours in an Exchange Economy.

We, of course, are not Robinson Crusoes living or working in isolation but the essential principles upon which a discussion of reduced working hours should first be based are identical with those outlined above for Crusoe. The fact that we are living in a society where exchange and specialised labour are fundamental institutions introduces, indeed, two important complications into our analysis. Firstly, the problem of elaborating new schedules of work presents itself now in the form of altering the proportions between the numbers of persons employed in various industries, and this may necessitate transfers of labour from one industry to another. It is quite easy for Crusoe to cut down the time spent on boat-building by half-an-hour and to make a corresponding increase in the amount of time spent on tending fresh vegetables or studying the remnants of the ship's library. It may be extremely difficult for an artisan, skilled in shipbuilding, to convert himself into a market gardener or a scientific research worker. Secondly, in a complex society such as ours, failure to solve quickly the problem of reallocating employment along the lines necessitated by changes in industrial technique does not merely impose the temporary inconvenience of an average level of production lower than is technically possible. It may also deprive some people of employment altogether and set in motion forces which tend to bring production to a level absolutely lower than had previously been attained and not merely lower relatively to what is possible.

The analysis which is relevant to the more complex situation in which we are actually living may also conveniently begin with the two-fold assumption (*a*) that the aggregate volume of employment is already distributed among the various industries and occupations in the most efficient way, and (*b*) that no factors are at work making possible a higher level of productivity. In such circumstances what should be our attitude to a claim for more leisure, for a reduction in the number of working hours ? Again the conclusion appears to be unavoidable that, if we accept our preliminary assumptions, such a claim can be met only if we are prepared to allow the volume of production to fall. To insist that hours shall be reduced and the customary level of income shall be maintained means in such circumstances a defence of two contradictory propositions. It might indeed be argued that the customary wage level could be maintained if other incomes were reduced. A decision as to whether this was either possible or desirable would, however, depend on other considerations not at all closely connected with the length of the working day. If there is a valid case for redistribution of income, the redistribution should be carried out without any regard to what happens to the working day. It might, of course, be quite sensible to insist that working hours should be reduced even if such a policy meant a lowering of production standards ; a community which deliberately made that choice could not reasonably be reproached for regarding other ends as more important than material production, but it should at least know what it was doing.

IV

Shorter Hours and Purchasing Power.

It is sometimes urged that the argument above misses the main point in regard to the maintenance of customary wage-levels when working hours have been reduced. The real advantage, it is argued, arises from the stimulus which will be given to effective purchasing power. It must be admitted that it is not a conclusive answer to this argument merely to point out that there is no *net* increase in the aggregate of purchasing power if we merely transfer money income from one section of the community to another, e.g., from employers to wage-earners. The argument is more subtle than this. In certain important circumstances it is true that a significant proportion of the community's purchasing power remains ineffective because of the tendency to hoard savings instead of converting them at an appropriate rate into useful instruments of production. Or, as our argument later will suggest, savings are wasted as a result of a chronic tendency to over-invest in staple well-established industries and a corresponding reluctance to take the risks of experimentation in the newer types of production which are necessary in a progressive economy if equilibrium is to be maintained and depression avoided. The transference of purchasing power to wage-earners will, it is claimed, reduce the volume of savings which are likely to be misapplied in this way, or to be merely hoarded.

The deflationary effects which follow failure to convert savings into useful productive equipment at an appropriate rate are, without doubt, an important contributory cause of persistent depression, but, on the whole, it seems likely that this problem will be more successfully handled by action which has a more direct impact upon the causes of the trouble than a reduction of hours. It seems a little rash to assume that the hypothetical correction which would follow a transfer of purchasing power from those employers who happened to find special difficulties in adjusting their plant to a reduction of working hours would do much to diminish the volume of unused or misused savings. It would be at least equally likely to diminish the volume of savings which was being properly applied. There is no obvious reason for supposing that the individuals who are responsible for savings which are unused or misused during a period of depression will be identical with the persons whose money incomes will be diminished if nominal wage rates are unaltered when working hours are reduced. It seems more satisfactory to base the claim for shorter hours on other foundations.

V

Shorter Hours as a Cause of Increased Productive Efficiency.

The case for a reduction of working hours appears much stronger if it is reasonable to suppose that it would initiate such favourable changes in the efficiency of production as would make possible an increased output per unit of time worked. If this hypothesis were well founded it would be possible to reduce hours and at the same time to maintain or even to raise the old level of production. There are many historic examples which justify this view and the case against further reduction of hours at the present time becomes much less plausible when we recall the fact that employers as a whole have always steadily refused to admit that in any circumstances a shorter working day or a shorter working week could increase productive efficiency. Crude arithmetical calculations have frequently led them to overestimate the length of the optimum working day and to oppose changes which later experience showed to be desirable from every standpoint. Mr. J. R. Hicks, who is by no means disposed to take an over-favourable view of the effects of "artificial" regulation of wages and working conditions, points out that "probably," in the days of the Industrial Revolution, "it had never entered the heads of most employers that it was at all conceivable that hours could be shortened and output maintained."[1] This obtuseness has not been confined to the early days of the Industrial Revolution. "The general principle underlying legislation to limit the hours of work in industry is sound."[2]

To what extent it would be reasonable to-day to expect shorter working hours to produce favourable reactions upon productive capacity is not a subject where confident generalisation is possible. This is a matter upon which the industrial psychologist should be best qualified to speak and it is unlikely that the reactions in different industries will be uniform. According to P. Sargant Florence in 1924[3] "reduction of hours below eight per day does not increase hourly output sufficiently to increase the daily total, unless, possibly, speed depends purely on the human factor and work is of a heavy type." It is, however, relevant to point out there are no valid reasons for supposing that when once the optimum working day or working week has been determined it will never again be varied. "As industry develops, the strain to which workpeople are exposed probably increases; rest and recreation become more necessary; and thus the output optimum length of day probably falls. If output is to be maintained at the maximum possible, hours ought to be reduced."[4] Even if we are prepared to accept without question the findings of the industrial psychologist, we must take care that these findings are based on the conditions prevailing to-day and not on the conditions of ten or twenty years ago.

A much more important point, however, arises here. We saw that if Robinson Crusoe discovered that a reduction in his working hours increased his efficiency he would be compelled to re-order his schedule of work, reducing the time allotted for certain tasks and increasing that which he allowed to others. In exactly the same way a community which found that efficiency was increased by reducing working hours, would be compelled to re-allocate its working population, employing fewer people in the industries where efficiency was most increased, and more people in the industries which supplied the goods and services which a community with increased capacity for wealth would find that it was now able to purchase. From this point of view, the discovery that a shorter working day in some particular industry will increase the efficiency of production is exactly similar to any other scientific discovery or invention. A reduction in working hours is obviously justified if it increases efficiency, but by itself it does nothing to diminish the volume of unemployment. Even if we were prepared to sacrifice altogether the increased production which increased efficiency made possible, we could avoid the necessity for re-allocating the working population only if we were free to determine the number of working hours in each industry without any reference to the number of hours which were being worked in other industries.

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[1] *The Theory of Wages*, p. 107.

[2] A. C. Pigou, *Economics of Welfare*, second edition, p. 433.

[3] *Economics of Fatigue and Unrest*, p. 348.

[4] J. R. Hicks, op. cit., p. 106.

Theoretically it is not inconceivable that the normal working day might be of quite different lengths in different industries. There are in fact at the present time some well-known divergences of this kind. In general, however, it is obviously necessary that any further reduction of hours should be applied to industry more or less as a whole.[1] This means that while a diminution in fatigue and in the risks of industrial accident might be held to justify a reduction of hours for steel workers only, it would be in practice necessary to allow a similar reduction to barbers, the quantity and efficiency of whose work could scarcely be much improved by any reduction in their normal working day. In order merely to maintain the existing level of production, without attempting to raise it, it would therefore be necessary to associate with the reduction of hours a redistribution of employment, diverting some steel workers into the hairdressing trade; if, as there is good reason for believing, it is precisely resistance to such redistribution which is one of the major causes of the unemployment for which a remedy is being sought, the difficulty of making this redistribution rather weakens the case for solving the unemployment problem by a reduction of hours.

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[1] Cf. Chief Judge Dethridge in Glass Workers' case, 1927. "It would not be sound practice to grant, in the case of workers employed by a monopoly, prosperous as it may be, standard working hours different from those of workers of a similar kind working for other employers, To do so would create discontent and unrest among those other workers." Cit., G. Anderson. *Fixation of Wages*, p. 530.

VI

SHORTER HOURS AS A CONSEQUENCE OF INCREASED PRODUCTIVE EFFICIENCY.

A consideration of the possibility of shorter hours increasing productive efficiency has already brought us nearer to the conditions of the real world where we must abandon our second hypothesis which assumed the absence of any new factors which make possible a higher level of productivity. The case for reducing working hours is of course urged strongly at the present time on the ground that such factors are and for a long time have been present ; because production has become so much more efficient, it is argued, we are justified in insisting upon the enjoyment of more leisure. The development of science and the rapidly extending use of new technical devices have greatly increased industrial efficiency, and it is argued that adjustment to the new conditions is impossible, or at least difficult, if normal working hours are not reduced.

Faced with conditions such as these the world has before it two alternatives. It can reorganise industry in such a way as to increase the volume and variety of output, thus making possible a general raising of standards of living, or it can maintain output at the old level and enjoy the advantages offered by a more efficient control of the environment in the form of greater leisure, of a reduction in working hours. The two alternatives are not, of course, mutually exclusive.

If the first alternative is preferred the world is then faced with the difficult problem of reallocating its productive resources, its labour and its capital, so that an appropriate number of workers and an appropriate supply of equipment will be available to produce the things which are necessary for higher standards of living. The problem is difficult without doubt, but the fact that standards of living have frequently been raised in the past proves that it is not insoluble. Many people, however, are unduly alarmed by the difficulties and, for this or other reasons, are disposed at the present time to prefer the second alternative, to take out the benefits of scientific advance exclusively in the form of more leisure.

Again it may be said that, provided we know what we are doing, this preference is not to be condemned. It is certainly not for people who are fortunate enough to be engaged in work which arouses spontaneous interest to despise the man who, condemned to monotonous routine toil, eagerly grasps the opportunity for enjoying a little more leisure. But, it must also be clearly understood, the belief that by concentrating upon a reduction of the working day we can avoid the necessity for inconvenient transfers of capital and labour is based upon an illusion. The discovery and general application of new inventions may justify a reduction of working hours, but it is highly improbable that the saving of time which the new inventions make possible will be uniform in all industries. If it is not, the necessity for re-allocating our labour forces can be avoided only if we are prepared to allow markedly unequal normal working days in different industries. "Let us examine an imaginary case, the figures in which have been selected to facilitate simple calculations. Suppose that there are only three industries, A, B and C, each employing 350,000 people, all of whom work eight hours per day. An invention in A then makes it possible to reduce the working time in that industry by three-eighths. If it were decided to realise all the advantages of this invention in greater leisure, the working day in A could be reduced to five hours. Why, however, should those who happened to be working in A receive the whole of the benefit? The case for a general reduction of hours, and not a reduction confined only to A, would, quite properly, be regarded as much more convincing. In the circumstances postulated the same aggregate volume of production could be maintained in a seven-hour day if there were 250,000 workers in A and 400,000 in each of B and C. This would clearly necessitate the transfer from A to B and C of 100,000 workers, a number possibly greater than the number which would have to change if a general increase in production had been preferred to greater leisure. In that case there would certainly have been some expansion of demand for the cheapened product of A and therefore less necessity for the transfer of workers from A."[1]

If then, whatever we do, we cannot evade the problem of transferring part of our labour resources from one industry to another, the most sensible course would appear to be one which combined the advantages of increased leisure with those of rising standards of living. It is not uncommon in these days to sigh for the alleged benefits which are believed to flow from the simpler civilisation of earlier times, and to suggest that we would do better to abandon the feverish struggle for better and more complicated standards of living. But it is significant that these complaints are most frequently found in the mouths of people whose standards of comfort are already well above the average and who seldom have any serious intention of allowing their own standards to be lowered. The simpler life which they contemplate is as a rule intended for other people; when we observe the unsatisfactory character of the average standards which prevail to-day, even in the wealthiest countries, standards of housing, of education, and of cultural development, it seems clear that we are still far distant from the day when we might be justified in calling a halt to the further advance of material progress. We might indeed, if the efficiency of production increased, reduce working hours at the same time as we enabled new types of production to expand by making available for them new types of labour and capital. It is true, moreover, that the greater our preference for leisure and the less our eagerness to try out new types of satisfaction, the smaller will be the number of individuals who will have to change their occupation, provided always that improvements in efficiency are allowed to have their normal effects in lowering costs and stimulating additional demand. But even in these circumstances the number of transfers that are necessary is still considerable, so that even if we sacrifice a large part of the material gains which might be enjoyed we are not able to evade the problem of transferring resources.[2]

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[1] A. G. B. Fisher, *Clash of Progress and Security*, pp. 123–4.

[2] It can be shown by a simple arithmetical computation that in the imaginary illustration quoted above, the number of necessary transfers would be reduced, if a lower price for the products of 'A' stimulated demand to such an extent that that output was increased by 20 per cent. from 100,000 to 63,636, but in that case the normal working day could be reduced by only 40 minutes. No transfers of labour would be necessary only where the demand for the products of A extended so far as to make it possible for all those originally employed in A to continue to find work there, and in such circumstances, of course, there will be no question of reducing working hours.

Whatever be the position in regard to these points in particular cases, the risk of temporary increase in costs which so much alarms employers when reduced working hours are suggested would certainly be much diminished if vigorous efforts were being made to transfer to new fields the labour and capital which technical advance was rendering superfluous in the old. The employers' case against reduced working hours on the ground that it is likely to increase production costs remains, broadly speaking, unchanged but a radically new factor is introduced if, at the same time as working hours are being reduced, the efficiency of production is increasing. In many instances the burden of excessive costs which weighs so heavily upon employers is an expression of the fact that industries are working with equipment which appears to be redundant in relation to the slackened demand characteristic of periods of depression, or which actually is redundant in the light of technical improvements which directly affect the industry in question. The general restoration of industrial activity which would follow the diversion of the labour and capital into the right channels would stimulate demand for the products of industries in the former class so that the overhead charges per unit of output would fall, while the units which were entitled to survive in industries of the latter class would no longer be handicapped by the conditions of glut which prevail so long as less favourably situated rivals are desperately striving to avoid the necessity for transferring to other fields. A reduction of working hours which at present appears to employers merely to increase costs at a time when costs are already too high could then more easily be seen in true perspective as a rational consequence of increased efficiency which made possible a reduction in costs.

In an ideal economy the two processes of reducing working hours and opening up new types of industrial activity would go on simultaneously side by side. At first sight one might be inclined to say that the reduction of working hours which was such a striking feature of the economic history of the 19th century was made possible by the technical improvements which then took place in many of the staple industries. It was not, however, these technical improvements *per se* which were so significant as the combination of technical improvements in certain fields of industry with a readiness to face the risks involved in exploring new industries. If investors generally are so much afflicted by caution as to refuse to face these risks to-day, the prospects for satisfactory adjustment to reduced working hours are much less bright.

Unfortunately we are not living in an ideal economy. Striking improvements in the technique of production have already occurred but we have been slow to make the necessary adjustments for opening up the new fields of production which are demanded by higher standards of living. This is indeed one important cause of the persistence of depression ; in consequence of too rigid adherence to the traditional distribution of labour and capital, industrial activity in general threatens to contract instead of expanding and the difficulties in the way of an effective reduction of working hours are accordingly much increased. A policy which might have been adopted with a considerable measure of success, and without causing undue upset, at a time when a prompt response to the necessity for transferring resources was maintaining industry as a whole upon an even keel, becomes much more risky for the individual entrepreneur when industrial development has been checked by a failure to make these responses, and producers everywhere feel themselves oppressed by the burden of high costs. The close association between the two policies however remains, and eager partisans of reduced working hours should be careful to insist with at least equal emphasis upon the necessity for accelerating the transfer of productive resources into new or comparatively new industries. The ideal time for reducing working hours is just at the moment when the effects of new technical devices are beginning to be felt. Unfortunately it is unlikely that there will be any speedy recognition of this fact. If, at the same time as a reduction of working hours is delayed the other adjustments demanded by technical improvements are also not carried out, industry is likely to become generally depressed and entrepreneurs, looking desperately around for directions along which costs can be reduced, will be very reluctant to subject themselves to the further risk of increased costs which shorter working hours might impose. Failure to make the necessary adjustments and the consequences which follow such failure do not indeed invalidate the thesis that increased productive efficiency may reasonably entitle us to the enjoyment of greater leisure, but unless the other adjustments are being carried out simultaneously, the practical difficulties in the way of shortening hours in particular industries may well appear to be overwhelming.

If this argument is sound the reduction of working hours is much less an independent instrument for helping us out of the depression than a consequence, probably socially advantageous and economically justifiable, of rapid improvements in productive technique to which our economy has been slow in adjusting itself. One further consideration should, however, be noted. Our argument has implied above that when the efficiency of production increases we are free to choose whether we shall enjoy the benefits exclusively in the form of increased production or exclusively in the form of more leisure or whether we shall compromise between these two objectives and enjoy something of each. At the stage of development which we have reached to-day it is doubtful, however, whether the first alternative is a real one. Increased production must now mean more and more production of a vast variety of miscellaneous goods and services which cannot be enjoyed except by people with much more leisure than was customary in the past. If therefore we want to develop a market for these things we must at the same time increase the opportunities for leisure and our efforts to increase and diversify production will fail unless working hours are reduced. " An increasing amount of leisure for the workers is now becoming an economic necessity. Without it they cannot possibly consume those secondary goods and services the production of which is likely to create more and more employment in the future."[1] This consideration greatly strengthens the case for shorter working hours but at the same time it also confirms the conclusion that shorter working hours must be combined with the development of new fields of production.

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[1] *Economist*, June 2nd, 1934, p. 1178.

The Nature of the Firm

By R. H. Coase

Economic theory has suffered in the past from a failure to state clearly its assumptions. Economists in building up a theory have often omitted to examine the foundations on which it was erected. This examination is, however, essential not only to prevent the misunderstanding and needless controversy which arise from a lack of knowledge of the assumptions on which a theory is based, but also because of the extreme importance for economics of good judgment in choosing between rival sets of assumptions. For instance, it is suggested that the use of the word " firm " in economics may be different from the use of the term by the " plain man."[1] Since there is apparently a trend in economic theory towards starting analysis with the individual firm and not with the industry,[2] it is all the more necessary not only that a clear definition of the word " firm " should be given but that its difference from a firm in the " real world," if it exists, should be made clear. Mrs. Robinson has said that " the two questions to be asked of a set of assumptions in economics are : Are they tractable ? and : Do they correspond with the real world ? "[3] Though, as Mrs. Robinson points out, " more often one set will be manageable and the other realistic," yet there may well be branches of theory where assumptions may be both manageable and realistic. It is hoped to show in the following paper that a definition of a firm may be obtained which is not only realistic in that it corresponds to what is meant by a firm in the real world, but is tractable by two of the most powerful instruments of economic analysis developed by Marshall, the idea of the margin and that of substitution, together giving the idea of substitution at the margin.[1] Our definition must, of course, "relate to formal relations which are capable of being *conceived* exactly."[2]

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[1] Joan Robinson, *Economics is a Serious Subject*, p. 12.
[2] See N. Kaldor, " The Equilibrium of the Firm," *Economic Journal*, March, 1934.
[3] Op. cit., p. 6.

I

It is convenient if, in searching for a definition of a firm, we first consider the economic system as it is normally treated by the economist. Let us consider the description of the economic system given by Sir Arthur Salter.[3] "The normal economic system works itself. For its current operation it is under no central control, it needs no central survey. Over the whole range of human activity and human need, supply is adjusted to demand, and production to consumption, by a process that is automatic, elastic and responsive." An economist thinks of the economic system as being co-ordinated by the price mechanism and society becomes not an organisation but an organism.[4] The economic system "works itself." This does not mean that there is no planning by individuals. These exercise foresight and choose between alternatives. This is necessarily so if there is to be order in the system. But this theory assumes that the direction of resources is dependent directly on the price mechanism. Indeed, it is often considered to be an objection to economic planning that it merely tries to do what is already done by the price mechanism.[5] Sir Arthur Salter's description, however, gives a very incomplete picture of our economic system. Within a firm, the description does not fit at all. For instance, in economic theory we find that the allocation of factors of production between different uses is determined by the price mechanism. The price of factor A becomes higher in X than in Y. As a result, A moves from Y to X until the difference between the prices in X and Y, except in so far as it compensates for other differential advantages, disappears. Yet in the real world, we find that there are many areas where this does not apply. If a workman moves from department Y to department X, he does not go because of a change in relative prices, but because he is ordered to do so. Those who object to economic planning on the grounds that the problem is solved by price movements can be answered by pointing out that there is planning within our economic system which is quite different from the individual planning mentioned above and which is akin to what is normally called economic planning. The example given above is typical of a large sphere in our modern economic system. Of course, this fact has not been ignored by economists. Marshall introduces organisation as a fourth factor of production; J. B. Clark gives the co-ordinating function to the entrepreneur; Professor Knight introduces managers who co-ordinate. As D. H. Robertson points out, we find "islands of conscious power in this ocean of unconscious co-operation like lumps of butter coagulating in a pail of buttermilk."[1] But in view of the fact that it is usually argued that co-ordination will be done by the price mechanism, why is such organisation necessary? Why are there these "islands of conscious power"? Outside the firm, price movements direct production, which is co-ordinated through a series of exchange transactions on the market. Within a firm, these market transactions are eliminated and in place of the complicated market structure with exchange transactions is substituted the entrepreneur-co-ordinator, who directs production.[2] It is clear that these are alternative methods of co-ordinating production. Yet, having regard to the fact that if production is regulated by price movements, production could be carried on without any organisation at all, well might we ask, why is there any organisation?

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[1] J. M. Keynes, *Essays in Biography*, pp. 223-4.

[2] L. Robbins, *Nature and Significance of Economic Science*, p. 63.

[3] This description is quoted with approval by D. H. Robertson, *Control of Industry*, p. 85, and by Professor Arnold Plant, "Trends in Business Administration," ECONOMICA, February, 1932. It appears in *Allied Shipping Control*, pp. 16-17.

[4] See F. A. Hayek, "The Trend of Economic Thinking," ECONOMICA, May, 1933.

[5] See F. A. Hayek, op. cit.

Of course, the degree to which the price mechanism is superseded varies greatly. In a department store, the allocation of the different sections to the various locations in the building may be done by the controlling authority or it may be the result of competitive price bidding for space. In the Lancashire cotton industry, a weaver can rent power and shop-room and can obtain looms and yarn on credit.[3] This co-ordination of the various factors of production is, however, normally carried out without the intervention of the price mechanism. As is evident, the amount of "vertical" integration, involving as it does the supersession of the price mechanism, varies greatly from industry to industry and from firm to firm.

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[1] Op. cit., p. 85.

[2] In the rest of this paper I shall use the term entrepreneur to refer to the person or persons who, in a competitive system, take the place of the price mechanism in the direction of resources.

[3] *Survey of Textile Industries*, p. 26.

It can, I think,* be assumed that the distinguishing mark of the firm is the supersession of the price mechanism. It is, of course, as Professor Robbins points out, “ related to an outside network of relative prices and costs,”[1] but it is important to discover the exact nature of this relationship. This distinction between the allocation of resources in a firm and the allocation in the economic system has been very vividly described by Mr. Maurice Dobb when discussing Adam Smith's conception of the capitalist: *“ It began to be seen that there was something more important than the relations inside each factory or unit captained by an undertaker; there were the relations of the undertaker with the rest of the economic world outside his immediate sphere . . . . the undertaker busies himself with the division of labour inside each firm and he plans and organises consciously,” but “ he is related to the much larger economic specialisation, of which he himself is merely one specialised unit. Here, he plays his part as a single cell in a larger organism, mainly unconscious of the wider rôle he fills.”[2]

In view of the fact that while economists treat the price mechanism as a co-ordinating instrument, they also admit the co-ordinating function of the “ entrepreneur,” it is surely important to enquire why co-ordination is the work of the price mechanism in one case and of the entrepreneur in another. *The purpose of this paper is to bridge what appears to be a gap in economic theory between the assumption (made for some purposes) that resources are allocated by means of the price mechanism and the assumption (made for other purposes) that this allocation is dependent on the entrepreneur-co-ordinator. We have to explain the basis on which, in practice, this choice between alternatives is effected.[3]

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[1] Op. cit., p. 71.

[2] *Capitalist Enterprise and Social Progress*, p. 20. Cf., also, Henderson, *Supply and Demand*, pp. 3–5.

[3] It is easy to see when the State takes over the direction of an industry that, in planning it, it is doing something which was previously done by the price mechanism. What is usually not realised is that any business man in organising the relations between his departments is also doing something which could be organised through the price mechanism. There is therefore point in Mr. Durbin's answer to those who emphasise the problems involved in economic planning that the same problems have to be solved by business men in the competitive system. (See “ Economic Calculus in a Planned Economy,” *Economic Journal*, December, 1936.) The important difference between these two cases is that economic planning is imposed on industry while firms arise voluntarily because they represent a more efficient method of organising production. In a competitive system, there is an “ optimum ” amount of planning!

II

Our task is to attempt to discover why a firm emerges at all in a specialised exchange economy. The price mechanism (considered purely from the side of the direction of resources) might be superseded if the relationship which replaced it was desired for its own sake. This would be the case, for example, if some people preferred to work under the direction of some other person. Such individuals would accept less in order to work under someone, and firms would arise naturally from this. But it would appear that this cannot be a very important reason, for it would rather seem that the opposite tendency is operating if one judges from the stress normally laid on the advantage of "being one's own master."[1] Of course, if the desire was not to be controlled but to control, to exercise power over others, then people might be willing to give up something in order to direct others; that is, they would be willing to pay others more than they could get under the price mechanism in order to be able to direct them. But this implies that those who direct pay in order to be able to do this and are not paid to direct, which is clearly not true in the majority of cases.[2] Firms might also exist if purchasers preferred commodities which are produced by firms to those not so produced; but even in spheres where one would expect such preferences (if they exist) to be of negligible importance, firms are to be found in the real world.[3] Therefore there must be other elements involved.

The main reason why it is profitable to establish a firm would seem to be that there is a cost of using the price mechanism. The most obvious cost of "organising" production through the price mechanism is that of discovering what the relevant prices are.[4] This cost may be reduced but it will not be eliminated by the emergence of specialists who will sell this information. The costs of negotiating and concluding a separate contract for each exchange transaction which takes place on a market must also be taken into account.[1] Again, in certain markets, e.g., produce exchanges, a technique is devised for minimising these contract costs; but they are not eliminated. It is true that contracts are not eliminated when there is a firm but they are greatly reduced. A factor of production (or the owner thereof) does not have to make a series of contracts with the factors with whom he is co-operating within the firm, as would be necessary, of course, if this co-operation were as a direct result of the working of the price mechanism. For this series of contracts is substituted one. At this stage, it is important to note the character of the contract into which a factor enters that is employed within a firm. The contract is one whereby the factor, for a certain remuneration (which may be fixed or fluctuating), agrees to obey the directions of an entrepreneur *within certain limits*.[2] The essence of the contract is that it should only state the limits to the powers of the entrepreneur. Within these limits, he can therefore direct the other factors of production.

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[1] Cf. Harry Dawes, "Labour Mobility in the Steel Industry," *Economic Journal*, March, 1934, who instances "the trek to retail shopkeeping and insurance work by the better paid of skilled men due to the desire (often the main aim in life of a worker) to be independent" (p. 86).

[2] None the less, this is not altogether fanciful. Some small shopkeepers are said to earn less than their assistants.

[3] G. F. Shove, "The Imperfection of the Market: a Further Note," *Economic Journal*, March, 1933, p. 116, note 1, points out that such preferences may exist, although the example he gives is almost the reverse of the instance given in the text.

[4] According to N. Kaldor, "A Classificatory Note of the Determinateness of Equilibrium," *Review of Economic Studies*, February, 1934, it is one of the assumptions of static theory that "All the relevant prices are known to all individuals." But this is clearly not true of the real world.

There are, however, other disadvantages—or costs—of using the price mechanism. It may be desired to make a long-term contract for the supply of some article or service. This may be due to the fact that if one contract is made for a longer period, instead of several shorter ones, then certain costs of making each contract will be avoided. Or, owing to the risk attitude of the people concerned, they may prefer to make a long rather than a short-term contract. Now, owing to the difficulty of forecasting, the longer the period of the contract is for the supply of the commodity or service, the less possible, and indeed, the less desirable it is for the person purchasing to specify what the other contracting party is expected to do. It may well be a matter of indifference to the person supplying the service or commodity which of several courses of action is taken, but not to the purchaser of that service or commodity. But the purchaser will not know which of these several courses he will want the supplier to take. Therefore, the service which is being provided is expressed in general terms, the exact details being left until a later date. All that is stated in the contract is the limits to what the persons supplying the commodity or service is expected to do. The details of what the supplier is expected to do is not stated in the contract but is decided later by the purchaser. When the direction of resources (within the limits of the contract) becomes dependent on the buyer in this way, that relationship which I term a " firm " may be obtained.[1] A firm is likely therefore to emerge in those cases where a very short term contract would be unsatisfactory. It is obviously of more importance in the case of services—labour—than it is in the case of the buying of commodities. In the case of commodities, the main items can be stated in advance and the details which will be decided later will be of minor significance.

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[1] This influence was noted by Professor Usher when discussing the development of capitalism. He says: "The successive buying and selling of partly finished products were sheer waste of energy." (*Introduction to the Industrial History of England*, p. 13). But he does not develop the idea nor consider why it is that buying and selling operations still exist.

[2] It would be possible for no limits to the powers of the entrepreneur to be fixed. This would be voluntary slavery. According to Professor Batt, *The Law of Master and Servant*, p. 18, such a contract would be void and unenforceable.

We may sum up this section of the argument by saying that the operation of a market costs something and by forming an organisation and allowing some authority (an " entrepreneur ") to direct the resources, certain ~~marketing~~ costs ~~are saved~~. The entrepreneur has to carry out his function at less cost, taking into account the fact that he may get factors of production at a lower price than the market transactions which he supersedes, because it is always possible to revert to the open market if he fails to do this.

The question of uncertainty is one which is often considered to be very relevant to the study of the equilibrium of the firm. It seems improbable that a firm would emerge without the existence of uncertainty. But those, for instance, Professor Knight, who make the *mode of payment* the distinguishing mark of the firm—fixed incomes being guaranteed to some of those engaged in production by a person who takes the residual, and fluctuating, income—would appear to be introducing a point which is irrelevant to the problem we are considering. One entrepreneur may sell his services to another for a certain sum of money, while the payment to his employees may be mainly or wholly a share in profits.[2] The significant question would appear to be why the allocation of resources is not done directly by the price mechanism.

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[1] Of course, it is not possible to draw a hard and fast line which determines whether there is a firm or not. There may be more or less direction. It is similar to the legal question of whether there is the relationship of master and servant or principal and agent. See the discussion of this problem below.

[2] The views of Professor Knight are examined below in more detail.

Another factor that should be noted is that exchange transactions on a market and the same transactions organised within a firm are often treated differently by Governments or other bodies with regulatory powers. If we consider the operation of a sales tax, it is clear that it is a tax on market transactions and not on the same transactions organised within the firm. Now since these are alternative methods of "organisation"—by the price mechanism or by the entrepreneur—such a regulation would bring into existence firms which otherwise would have no *raison d'être*. It would furnish a reason for the emergence of a firm in a specialised exchange economy. Of course, to the extent that firms already exist, such a measure as a sales tax would merely tend to make them larger than they would otherwise be. Similarly, quota schemes, and methods of price control which imply that there is rationing, and which do not apply to firms producing such products for themselves, by allowing advantages to those who organise within the firm and not through the market, necessarily encourage the growth of firms. But it is difficult to believe that it is measures such as have been mentioned in this paragraph which have brought firms into existence. Such measures would, however, tend to have this result if they did not exist for other reasons.

These, then, are the reasons why organisations such as firms exist in a specialised exchange economy in which it is generally assumed that the distribution of resources is "organised" by the price mechanism. A firm, therefore, consists of the system of relationships which comes into existence when the direction of resources is dependent on an entrepreneur.

The approach which has just been sketched would appear to offer an advantage in that it is possible to give a scientific meaning to what is meant by saying that a firm gets larger or smaller. A firm becomes larger as additional transactions (which could be exchange transactions co-ordinated through the price mechanism) are organised by the entrepreneur and becomes smaller as he abandons the organisation of such transactions. The question which arises is whether it is possible to study the forces which determine the size of the firm. Why does the entrepreneur not organise one less transaction or one more ? It is interesting to note that Professor Knight considers that :

> " the relation between efficiency and size is one of the most serious problems of theory, being, in contrast with the relation for a plant, largely a matter of personality and historical accident rather than of intelligible general principles. But the question is peculiarly vital because the possibility of monopoly gain offers a powerful incentive to *continuous and unlimited* expansion of the firm, which force must be offset by some equally powerful one making for decreased efficiency (in the production of money income) with growth in size, if even boundary competition is to exist."[1]

Professor Knight would appear to consider that it is impossible to treat scientifically the determinants of the size of the firm. On the basis of the concept of the firm developed above, this task will now be attempted.

It was suggested that the introduction of the firm was due primarily to the existence of marketing costs. A pertinent question to ask would appear to be (quite apart from the monopoly considerations raised by Professor Knight), why, if by organising one can eliminate certain costs and in fact reduce the cost of production, are there any market transactions at all ?[2] Why is not all production carried on by one big firm ? There would appear to be certain possible explanations.

First, as a firm gets larger, there may be decreasing returns to the entrepreneur function, that is, the costs of organising additional transactions within the firm may rise.[3] Naturally, a point must be reached where the costs of organising an extra transaction within the firm are equal to the costs involved in carrying out the transaction in the open market, or, to the costs of organising by another entrepreneur. Secondly, it may be that as the transactions which are organised increase, the entrepreneur fails to place the factors of production in the uses where their value is greatest, that is, fails to make the best use of the factors of production. Again, a point must be reached where the loss through the waste of resources is equal to the marketing costs of the exchange transaction in the open market or to the loss if the transaction was organised by another entrepreneur. Finally, the supply price of one or more of the factors of production may rise, because the "other advantages" of a small firm are greater than those of a large firm.[1] Of course, the actual point where the expansion of the firm ceases might be determined by a combination of the factors mentioned above. The first two reasons given most probably correspond to the economists' phrase of "diminishing returns to management."[2]

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[1] *Risk, Uncertainty and Profit,* Preface to the Re-issue, London School of Economics Series of Reprints, No. 16, 1933.

[2] There are certain marketing costs which could only be eliminated by the abolition of " consumers' choice " and these are the costs of retailing. It is conceivable that these costs might be so high that people would be willing to accept rations because the extra product obtained was worth the loss of their choice.

[3] This argument assumes that exchange transactions on a market can be considered as homogeneous ; which is clearly untrue in fact. This complication is taken into account below.

The point has been made in the previous paragraph that a firm will tend to expand until the costs of organising an extra transaction within the firm become equal to the costs of carrying out the same transaction by means of an exchange on the open market or the costs of organising in another firm. But if the firm stops its expansion at a point below the costs of marketing in the open market and at a point equal to the costs of organising in another firm, in most cases (excluding the case of "combination"[3]), this will imply that there is a market transaction between these two producers, each of whom could organise it at less than the actual marketing costs. How is the paradox to be resolved? If we consider an example the reason for this will become clear. Suppose *A* is buying a product from *B* and that both *A* and *B* could organise this marketing transaction at less than its present cost. *B*, we can assume, is not organising one process or stage of production, but several. If *A* therefore wishes to avoid a market transaction, he will have to take over all the processes of production controlled by *B*. Unless *A* takes over all the processes of production, a market transaction will still remain, although it is a different product that is bought. But we have previously assumed that as each producer expands he becomes less efficient ; the additional costs of organising extra transactions increase. It is probable that *A*'s cost of organising the transactions previously organised by *B* will be greater than *B*'s cost of doing the same thing. *A* therefore will take over the whole of *B*'s organisation only if his cost of organising *B*'s work is not greater than *B*'s cost by an amount equal to the costs of carrying out an exchange transaction on the open market. But once it becomes economical to have a market transaction, it also pays to divide production in such a way that the cost of organising an extra transaction in each firm is the same.

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[1] For a discussion of the variation of the supply price of factors of production to firms of varying size, see E. A. G. Robinson, *The Structure of Competitive Industry*. It is sometimes said that the supply price of organising ability increases as the size of the firm increases because men prefer to be the heads of small independent businesses rather than the heads of departments in a large business. See Jones, *The Trust Problem*, p. 531, and Macgregor, *Industrial Combination*, p. 63. This is a common argument of those who advocate Rationalisation. It is said that larger units would be more efficient, but owing to the individualistic spirit of the smaller entrepreneurs, they prefer to remain independent, apparently in spite of the higher income which their increased efficiency under Rationalisation makes possible.

[2] This discussion is, of course, brief and incomplete. For a more thorough discussion of this particular problem, see N. Kaldor, "The Equilibrium of the Firm," *Economic Journal*, March, 1934, and E. A. G. Robinson, "The Problem of Management and the Size of the Firm," *Economic Journal*, June, 1934.

[3] A definition of this term is given below.

Up to now it has been assumed that the exchange transactions which take place through the price mechanism are homogeneous. In fact, nothing could be more diverse than the actual transactions which take place in our modern world. This would seem to imply that the costs of carrying out exchange transactions through the price mechanism will vary considerably as will also the costs of organising these transactions within the firm. It seems therefore possible that quite apart from the question of diminishing returns the costs of organising certain transactions within the firm may be greater than the costs of carrying out the exchange transactions in the open market. This would necessarily imply that there were exchange transactions carried out through the price mechanism, but would it mean that there would have to be more than one firm ? Clearly not, for all those areas in the economic system where the direction of resources was not dependent directly on the price mechanism could be organised within one firm. The factors which were discussed earlier would seem to be the important ones, though it is difficult to say whether " diminishing returns to management " or the rising supply price of factors is likely to be the more important.

Other things being equal, therefore, a firm will tend to be larger :

(*a*) the less the costs of organising and the slower these costs rise with an increase in the transactions organised.

(*b*) the less likely the entrepreneur is to make mistakes and the smaller the increase in mistakes with an increase in the transactions organised.

(*c*) the greater the lowering (or the less the rise) in the supply price of factors of production to firms of larger size.

Apart from variations in the supply price of factors of production to firms of different sizes, it would appear that the costs of organising and the losses through mistakes will increase with an increase in the spatial distribution of the transactions organised, in the dissimilarity of the transactions, and in the probability of changes in the relevant prices.[1] As more transactions are organised by an entrepreneur, it would appear that the transactions would tend to be either different in kind or in different places. This furnishes an additional reason why efficiency will tend to decrease as the firm gets larger. Inventions which tend to bring factors of production nearer together, by lessening spatial distribution, tend to increase the size of the firm.[2] Changes like the telephone and the telegraph which tend to reduce the cost of organising spatially will tend to increase the size of the firm. All changes which improve managerial technique will tend to increase the size of the firm.[3-4]

It should be noted that the definition of a firm which was given above can be used to give more precise meanings to the terms " combination " and " integration."[5] There is a combination when transactions which were previously organised by two or more entrepreneurs become organised by one. This becomes integration when it involves the organisation of transactions which were previously carried out between the entrepreneurs on a market. A firm can expand in either or both of these two ways. The whole of the " structure of competitive industry " becomes tractable by the ordinary technique of economic analysis.

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[1] This aspect of the problem is emphasised by N. Kaldor, op. cit. Its importance in this connection had been previously noted by E. A. G. Robinson, *The Structure of Competitive Industry*, pp. 83–106. This assumes that an increase in the probability of price movements increases the costs of organising within a firm more than it increases the cost of carrying out an exchange transaction on the market—which is probable.

[2] This would appear to be the importance of the treatment of the technical unit by E. A. G. Robinson, op. cit., pp. 27–33. The larger the technical unit, the greater the concentration of factors and therefore the firm is likely to be larger.

[3] It should be noted that most inventions will change both the costs of organising and the costs of using the price mechanism. In such cases, whether the invention tends to make firms larger or smaller will depend on the relative effect on these two sets of costs. For instance, if the telephone reduces the costs of using the price mechanism more than it reduces the costs of organising, then it will have the effect of reducing the size of the firm.

[4] An illustration of these dynamic forces is furnished by Maurice Dobb, *Russian Economic Development*, p. 68. " With the passing of bonded labour the factory, as an establishment where work was organised under the whip of the overseer, lost its *raison d'être* until this was restored to it with the introduction of power machinery after 1846." It seems important to realise that the passage from the domestic system to the factory system is not a mere historical accident, but is conditioned by economic forces. This is shown by the fact that it is possible to move from the factory system to the domestic system, as in the Russian example, as well as *vice versa*. It is the essence of serfdom that the price mechanism is not allowed to operate. Therefore, there has to be direction from some organiser. When, however, serfdom passed, the price mechanism was allowed to operate. It was not until machinery drew workers into one locality that it paid to supersede the price mechanism and the firm again emerged.

[5] This is often called " vertical integration," combination being termed " lateral integration."

III

The problem which has been investigated in the previous section has not been entirely neglected by economists and it is now necessary to consider why the reasons given above for the emergence of a firm in a specialised exchange economy are to be preferred to the other explanations which have been offered.

It is sometimes said that the reason for the existence of a firm is to be found in the division of labour. This is the view of Professor Usher, a view which has been adopted and expanded by Mr. Maurice Dobb. The firm becomes " the result of an increasing complexity of the division of labour . . . . The growth of this economic differentiation creates the need for some integrating force without which differentiation would collapse into chaos ; and it is as the integrating force in a differentiated economy that industrial forms are chiefly significant."[1] The answer to this argument is an obvious one. The " integrating force in a differentiated economy " already exists in the form of the price mechanism. It is perhaps the main achievement of economic science that it has shown that there is no reason to suppose that specialisation must lead to chaos.[2] The reason given by Mr. Maurice Dobb is therefore inadmissible. What has to be explained is why one integrating force (the entrepreneur) should be substituted for another integrating force (the price mechanism).

The most interesting reasons (and probably the most widely accepted) which have been given to explain this fact are those to be found in Professor Knight's *Risk, Uncertainty and Profit*. His views will be examined in some detail.

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[1] Op. cit., p. 10. Professor Usher's views are to be found in his *Introduction to the Industrial History of England*, pp. 1–18.

[2] Cf. J. B. Clark, *Distribution of Wealth*, p. 19, who speaks of the theory of exchange as being the " theory of the organisation of industrial society."

Professor Knight starts with a system in which there is no uncertainty:

> "acting as individuals under absolute freedom but without collusion men are supposed to have organised economic life with the primary and secondary division of labour, the use of capital, etc., developed to the point familiar in present-day America. The principal fact which calls for the exercise of the imagination is the internal organisation of the productive groups or establishments. With uncertainty entirely absent, every individual being in possession of perfect knowledge of the situation, there would be no occasion for anything of the nature of responsible management or control of productive activity. Even marketing transactions in any realistic sense would not be found. The flow of raw materials and productive services to the consumer would be entirely automatic."[1]

Professor Knight says that we can imagine this adjustment as being "the result of a long process of experimentation worked out by trial-and-error methods alone," while it is not necessary "to imagine every worker doing exactly the right thing at the right time in a sort of 'pre-established harmony' with the work of others. There might be managers, superintendents, etc., for the purpose of co-ordinating the activities of individuals," though these managers would be performing a purely routine function, "without responsibility of any sort."[2]

Professor Knight then continues:

> "With the introduction of uncertainty—the fact of ignorance and the necessity of acting upon opinion rather than knowledge—into this Eden-like situation, its character is entirely changed . . . . With uncertainty present doing things, the actual execution of activity, becomes in a real sense a secondary part of life; the primary problem or function is deciding what to do and how to do it."[3]

This fact of uncertainty brings about the two most important characteristics of social organisation.

> "In the first place, goods are produced for a market, on the basis of entirely impersonal prediction of wants, not for the satisfaction of the wants of the producers themselves. The producer takes the responsibility of

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[1] *Risk, Uncertainty and Profit*, p. 267.

[2] Op. cit., pp. 267–8.

[3] Op. cit., p. 268.

> forecasting the consumers' wants. In the second place, the work of forecasting and at the same time a large part of the technological direction and control of production are still further concentrated upon a very narrow class of the producers, and we meet with a new economic functionary, the entrepreneur. . . . . When uncertainty is present and the task of deciding what to do and how to do it takes the ascendancy over that of execution the internal organisation of the productive groups is no longer a matter of indifference or a mechanical detail. Centralisation of this deciding and controlling function is imperative, a process of 'cephalisation' is inevitable."[1]

> The most fundamental change is:

> "the system under which the confident and venturesome assume the risk or insure the doubtful and timid by guaranteeing to the latter a specified income in return for an assignment of the actual results. . . . With human nature as we know it it would be impracticable or very unusual for one man to guarantee to another a definite result of the latter's actions without being given power to direct his work. And on the other hand the second party would not place himself under the direction of the first without such a guarantee. . . . The result of this manifold specialisation of function is the enterprise and wage system of industry. Its existence in the world is the direct result of the fact of uncertainty."[2]

These quotations give the essence of Professor Knight's theory. The fact of uncertainty means that people have to forecast future wants. Therefore, you get a special class springing up who direct the activities of others to whom they give guaranteed wages. It acts because good judgment is generally associated with confidence in one's judgment.[3]

Professor Knight would appear to leave himself open to criticism on several grounds. First of all, as he himself points out, the fact that certain people have better judgment or better knowledge does not mean that they can only get an income from it by themselves actively taking part in production. They can sell advice or knowledge. Every business buys the services of a host of advisers. We can imagine a system where all advice or knowledge was bought as required. Again, it is possible to get a reward from better knowledge or judgment not by actively taking part in production but by making contracts with people who are producing. A merchant buying for future delivery represents an example of this. But this merely illustrates the point that it is quite possible to give a guaranteed reward providing that certain acts are performed without directing the performance of those acts. Professor Knight says that " with human nature as we know it it would be impracticable or very unusual for one man to guarantee to another a definite result of the latter's actions without being given power to direct his work." This is surely incorrect. A large proportion of jobs are done to contract, that is, the contractor is guaranteed a certain sum providing he performs certain acts. But this does not involve any direction. It does mean, however, that the system of relative prices has been changed and that there will be a new arrangement of the factors of production.[1] The fact that Professor Knight mentions that the " second party would not place himself under the direction of the first without such a guarantee " is irrelevant to the problem we are considering. Finally, it seems important to notice that even in the case of an economic system where there is no uncertainty Professor Knight considers that there would be co-ordinators, though they would perform only a routine function. He immediately adds that they would be " without responsibility of any sort," which raises the question by whom are they paid and why ? It seems that nowhere does Professor Knight give a reason why the price mechanism should be superseded.

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[1] Op. cit., pp. 268–95.

[2] Op. cit., pp. 269–70.

[3] Op. cit., p. 270.

IV

It would seem important to examine one further point and that is to consider the relevance of this discussion to the general question of the " cost-curve of the firm."

It has sometimes been assumed that a firm is limited in size under perfect competition if its cost curve slopes upward,[2] while under imperfect competition, it is limited in size because it will not pay to produce more than the output at which marginal cost is equal to marginal revenue.[1] But it is clear that a firm may produce more than one product and, therefore, there appears to be no *prima facie* reason why this upward slope of the cost curve in the case of perfect competition or the fact that marginal cost will not always be below marginal revenue in the case of imperfect competition should limit the size of the firm.[2] Mrs. Robinson[3] makes the simplifying assumption that only one product is being produced. But it is clearly important to investigate how the number of products produced by a firm is determined, while no theory which assumes that only one product is in fact produced can have very great practical significance.

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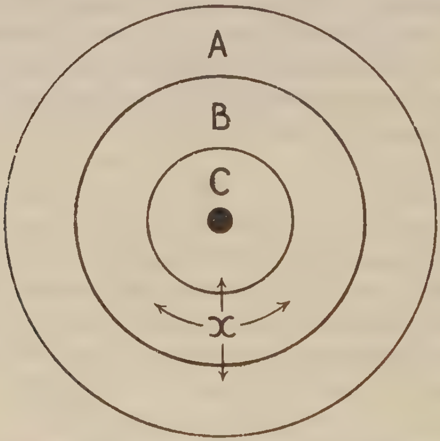
[1] This shows that it is possible to have a private enterprise system without the existence of firms. Though, in practice, the two functions of enterprise, which actually influences the system of relative prices by forecasting wants and acting in accordance with such forecasts, and management, which accepts the system of relative prices as being given, are normally carried out by the same persons, yet it seems important to keep them separate in theory. This point is further discussed below.

[2] See Kaldor, op. cit., and Robinson, *The Problem of Management and the Size of the Firm.*

It might be replied that under perfect competition, since everything that is produced can be sold at the prevailing price, then there is no need for any other product to be produced. But this argument ignores the fact that there may be a point where it is less costly to organise the exchange transactions of a new product than to organise further exchange transactions of the old product. This point can be illustrated in the following way. Imagine, following von Thunen, that there is a town, the consuming centre, and that industries are located around this central point in rings. These conditions are illustrated in the following diagram in which *A*, *B* and *C* represent different industries.



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[1] Mr. Robinson calls this the Imperfect Competition solution for the survival of the small firm.

[2] Mr. Robinson's conclusion, op. cit., p. 249, note 1, would appear to be definitely wrong. He is followed by Horace J. White, Jr., "Monopolistic and Perfect Competition," *American Economic Review*, December, 1936, p. 645, note 27. Mr. White states "It is obvious that the size of the firm is limited in conditions of monopolistic competition."

[3] *Economics of Imperfect Competition*.

Imagine an entrepreneur who starts controlling exchange transactions from x. Now as he extends his activities in the same product (B), the cost of organising increases until at some point it becomes equal to that of a dissimilar product which is nearer. As the firm expands, it will therefore from this point include more than one product (A and C). This treatment of the problem is obviously incomplete,[1] but it is necessary to show that merely proving that the cost curve turns upwards does not give a limitation to the size of the firm. So far we have only considered the case of perfect competition; the case of imperfect competition would appear to be obvious.

To determine the size of the firm, we have to consider the marketing costs (that is, the costs of using the price mechanism), and the costs of organising of different entrepreneurs and then we can determine how many products will be produced by each firm and how much of each it will produce. It would, therefore, appear that Mr. Shove[2] in his article on "Imperfect Competition" was asking questions which Mrs. Robinson's cost curve apparatus cannot answer. The factors mentioned above would seem to be the relevant ones.

V

Only one task now remains; and that is, to see whether the concept of a firm which has been developed fits in with that existing in the real world. We can best approach the question of what constitutes a firm in practice by considering the legal relationship normally called that of "master and servant" or "employer and employee."[3] The essentials of this relationship have been given as follows:

"(1) the servant must be under the duty of rendering personal services to the master or to others on behalf

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[1] As has been shown above, location is only one of the factors influencing the cost of organising.

[2] G. F. Shove, "The Imperfection of the Market," *Economic Journal*, March, 1933, p. 115. In connection with an increase in demand in the suburbs and the effect on the price charged by suppliers, Mr. Shove asks ". . . . why do not the old firms open branches in the suburbs?" If the argument in the text is correct, this is a question which Mrs. Robinson's apparatus cannot answer.

[3] The legal concept of "employer and employee" and the economic concept of a firm are not identical, in that the firm may imply control over another person's property as well as over their labour. But the identity of these two concepts is sufficiently close for an examination of the legal concept to be of value in appraising the worth of the economic concept.

> of the master, otherwise the contract is a contract for sale of goods or the like.
>
> (2) The master must have the right to control the servant's work, either personally or by another servant or agent. It is this right of control or interference, of being entitled to tell the servant when to work (within the hours of service) and when not to work, and what work to do and how to do it (within the terms of such service) which is the dominant characteristic in this relation and marks off the servant from an independent contractor, or from one employed merely to give to his employer the fruits of his labour. In the latter case, the contractor or performer is not under the employer's control in doing the work or effecting the service; he has to shape and manage his work so as to give the result he has contracted to effect."[1]

We thus see that it is the fact of direction which is the essence of the legal concept of "employer and employee," just as it was in the economic concept which was developed above. It is interesting to note that Professor Batt says further:

> "That which distinguishes an agent from a servant is not the absence or presence of a fixed wage or the payment only of commission on business done, but rather the freedom with which an agent may carry out his employment."[2]

We can therefore conclude that the definition we have given is one which approximates closely to the firm as it is considered in the real world.

Our definition is, therefore, realistic. Is it manageable? This ought to be clear. When we are considering how large a firm will be the principle of marginalism works smoothly. The question always is, will it pay to bring an extra exchange transaction under the organising authority? At the margin, the costs of organising within the firm will be equal either to the costs of organising in another firm or to the costs involved in leaving the transaction to be "organised" by the price mechanism. Business men will be constantly experimenting, controlling more or less, and in this way, equilibrium will be maintained. This gives the position of equilibrium for static analysis. But it is clear that the dynamic factors are also of considerable importance, and an investigation of the effect changes have on the cost of organising within the firm and on marketing costs generally will enable one to explain why firms get larger and smaller. We thus have a theory of moving equilibrium. The above analysis would also appear to have clarified the relationship between initiative or enterprise and management. Initiative means forecasting and operates through the price mechanism by the making of new contracts. Management proper merely reacts to price changes, rearranging the factors of production under its control. That the business man normally combines both functions is an obvious result of the marketing costs which were discussed above. Finally, this analysis enables us to state more exactly what is meant by the " marginal product " of the entrepreneur. But an elaboration of this point would take us far from our comparatively simple task of definition and clarification.

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[1] Batt, *The Law of Master and Servant*, p. 6.

[2] Op. cit., p. 7.

A New Approach to the Methodology of the Social Sciences

By ALFRED STONIER and KARL BODE

1. THE object of this article is to draw the attention of English readers to a work by Dr. Alfred Schütz which has important bearings on the methodological problems of social science.[1]

The subject matter and structure of Dr. Schütz's book may, in the first instance, perhaps best be indicated by analysing its title and sub-title, both most carefully and pointedly worded by the author. *Der sinnhafte Aufbau der sozialen Welt*. *Sinnhaft* means significant or " having meaning " ; *Aufbau* means structure or construction ; and *soziale Welt* might be translated by " the complex of social life ". As an " introduction to interpretative[2] sociology " this book contributes to the classification and analysis of the forms of social life, and—in its methodological aspect—to the provision of a systematic basis for the various social sciences.

Dr. Schütz takes as his starting-point the methodological writings of Max Weber[3] and he tries to develop further the system of analysis laid down by the latter more than a decade ago. In particular, the principle of methodological individualism and the principle of constructing ideal types are basic features which both these authors have in common. According to the first principle all social phenomena consist of the actions of individuals or of their results and all these actions must be defined primarily in terms of their subjective meaning, that is of how the actor himself regards them.

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[1] Alfred Schütz, *Der sinnhafte Aufbau der sozialen Welt* (*Eine Einleitung in die verstehende Soziologie*), Verlag Julius Springer, Wien, 1932. We are greatly indebted to Dr. Schütz who read our manuscript most carefully at one stage and corrected several mistakes of interpretation and emphasis. It must be stressed, however, that our exposition is not in any sense authoritative. Acknowledgments are also due to Mr. Hugh Gaitskell for helping to cast the argument in a form suitable for English readers.

[2] It should be noticed that *verstehen* has a rather wider meaning than " interpret." In some contexts it would be better translated by " understand."

[3] *Gesammelte Aufsätze zur Wissenschaftslehre* and *Wirtschaft und Gesellschaft*.

According to the second principle the social scientist, who tries to interpret social phenomena along these lines, constructs ideal types of such actions by abstracting elements common to different concrete examples and putting them together to form extreme or ideal cases.

But in contrast to Weber Dr. Schütz poses explicitly the problem how far it is possible, in scientific enquiry or in ordinary social life, to interpret adequately the subjective meaning of other people's actions. Moreover, unlike Weber, he believes that the key to all problems of meaning and interpretation is to be found in the Bergsonian concept of time or duration. He therefore conceives that by applying Bergson's analysis of human consciousness in general, which is worked out by Husserl on somewhat parallel lines but much more systematically and with direct reference to the genesis of meaning, Weber's system can be given a more solid foundation.

This is the first attempt to analyse Weber's basic problems in terms of Husserl's theory of time. The present work is not, however, a mere juxtaposition of the two systems or a mechanical application of one to the other, but on the contrary a most enlightening synthesis and development of both. Moreover Dr. Schütz's own contributions, in particular his distinctions between behaviour and action, between the in-order-to motive and because motive and between the different basic spheres of social life are of great importance for the systematic development of social science.

A brief survey of the table of contents will show the range of problems treated by Dr. Schütz. The book begins with a critical analysis of Weber's fundamental concepts in *Wirtschaft und Gesellschaft*. The second chapter deals, from the point of view of Husserl's time analysis, with the (subjective) meaning of "experiences",[1] and in particular of actions, in one's own individual consciousness. In the third chapter a theory is expounded of how one interprets other people's experiences, and in particular their actions. In the fourth chapter the concepts of social behaviour, social action and social relations are defined, and a classification of the social world is proposed in its different spheres as *Umwelt* (associates), *Mitwelt* (contemporaries), *Vorwelt* (predecessors) and *Folgewelt* (successors). The concluding chapter discusses some methodological questions of interpretative sociology.

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[1] It should be noted that in this context "experience" stands not for *Erfahrung* but for *Erlebnis*.

The following summary of the book is intended particularly for readers unacquainted with the German language and with contemporary German philosophical and sociological thought. It has therefore seemed advisable in a certain degree to simplify Dr. Schütz's own analysis and to omit altogether his criticisms of Weber. The present article is purely expository; our appraisal of the theory is left for another occasion.

2. In Dr. Schütz's view the fundamental problem of social science is the interpretation of the subjective meaning of social actions.

The first step in the argument is to define on the one hand the correlative concepts of meaning and interpretation, and on the other hand the concept of action, each without reference to the social sphere. The starting point for both problems is Bergson's and Husserl's theory of duration or psychic time.

What is immediately or ultimately given in the human consciousness is a continuous stream of experiences in which one phase passes gradually without definite breaks into another. Life is essentially this flow of continuous duration. It is fundamentally different from the world as composed of separate objects, which we apprehend under the purely conceptual forms of time (in the sense of *temps*) and of space. In ordinary life and thought this fact is overlooked because one does not distinguish between the pure data of consciousness and the world of objects which one uses implicitly as a frame of reference in interpreting them. In order to distinguish explicitly between the data of consciousness and such frames of reference one must abstract from the presuppositions of ordinary thought, which are pragmatically determined by the *attention à la vie*, and concentrate wholly on the immediate data of consciousness.

Now the contents of this stream of experience in continuous duration have no meaning in themselves. They just *are* or rather they gradually come into being and pass out of being, none being given as a distinct and separate entity. But when they have passed away it becomes possible for the individual, in an act of reflection, to " turn round " and separate one distinct experience from another. This act of reflection or recognition, which " identifies " experiences of past duration and makes them into determinate objects, gives them meaning in the most general sense. The meaning of an experience is its recognition by an act of reflection after it has itself passed out of existence, i.e., out of present duration.[1]

The details of this process of attributing meaning are very complicated even in the simplest case where only the meaning of a particular experience and not a whole complex of meaning is in question. For there are differences according to the point of time when the act of reflection sets in. One may reflect on a certain experience when, though no longer actually being experienced, it is still retained in what may be called primary memory. But later on, when both the impression and the retention of it have passed away, i.e., when the experience has been temporarily forgotten, it can be reproduced and reflected on in secondary memory.

We need not deal with these complications any further. For what follows it is sufficient to bear in mind the simple description of the process of ascribing meaning, and in particular the fact that—according to Dr. Schütz's theory—meaning can never be ascribed to actual experiences, that is to experiences actually present in duration.[2] It must be added however that, though the attribution of meaning in retrospect is the fundamental case, there is no obstacle to the attribution of meaning in prospect. The individual may perfectly well reflect upon an experience which he is going to have at a future date, e.g., the things he will probably see to-morrow on waking up. But even reflection about the future is in a sense reflection on the past. One can anticipate a future experience only in terms of similar past experiences, and one can therefore imagine it only as though it were already finished. Meaning is ascribed here, in Dr. Schütz's elegant terminology, in the future perfect tense (*modo futuri exacti*) as about to have been experienced.

Having reached a definition of the concept of meaning we must now turn to the analysis of action for, it will be remembered, all these subtleties were only dealt with for the sake of obtaining a clearly defined concept of the subjective meaning of actions. Here again the starting point is the Bergsonian stream of consciousness, but this time it is the fact that our experiences form two distinct groups, on the one hand passive and on the other hand active experiences.[1] As regards the former, external sense impressions such as the feeling of warmth are a standard example. For the latter we have already had an instance in the acts of reflection or recognition, which were considered, however, only in so far as they ascribe meaning to other past or future experiences. But they are of course experiences themselves, coming into being and passing out of being in the flow of continuous duration. These active experiences, clearly distinguished from the passive contents of consciousness by their spontaneous character, are called behaviour (*Verhalten*).[2] In accordance with the general rule, acts of behaviour have no meaning whilst being actually experienced. But as soon as they have passed away or are beginning to pass away, they may be reflected upon by other acts of behaviour and thus acquire meaning in the past tense.

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[1] Dr. Schütz does not attempt a detailed proof of this fundamental contention but refers the reader to Husserl. Those unacquainted with the writings of the latter may perhaps find a certain analogy in the ideas of Proust.

[2] Dr. Schütz distinguished a group of experiences intimately connected with the personality as being essentially actual experiences, to which therefore meaning can be ascribed neither in retrospect nor in prospect. They are, in the main, emotions and physiological sensations difficult to reproduce in secondary memory.

They may also be reflected upon in the future perfect tense. If, in this case, a deliberate act of intending joins the act of reflection, we get the most important kind of behaviour, viz., action (*Handlung*) the distinguishing characteristic of which is precisely that it is determined by a project which precedes it in time.[3] Action then is behaviour in accordance with a plan of projected behaviour ; and the project is neither more nor less than the action itself conceived, and determined on, in the future perfect as about to have been carried out. It is the unity of the project that constitutes the unity of an action, which, in the case of external bodily action, appears if looked at from the outside as a mere agglomeration of physical movements. Action may also be called conscious behaviour. Behaviour is of course a content of consciousness like all other experiences and it can also be made conscious in a narrower sense by being reflected on. But action is peculiar in being necessarily reflected on, in the project, before it is carried out. Thus the project is the primary and fundamental meaning of the action.

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[1] Dr. Schütz does not commit himself on the question whether any concrete experience can be purely passive or purely active. He is, however, rather inclined to deny the possibility of purely passive experiences. The above distinction must therefore be understood as one of degree, but as such it is highly important.

[2] The experience, for example, of feeling my leg moved by an external force—or the experience of a reflex movement—is very different from that of moving it (spontaneously) myself. It should be noted that the term " behaviour " is used here in a sense almost the opposite of that which it has acquired in behaviourist psychology. Not only " internal " but also external behaviour consists essentially not of physical movements but of the series of experiences which accompanies it. This is particularly important for deciding what constitutes the unity of an action.

[3] Dr. Schütz's distinction between action and other kinds of behaviour corresponds to that between impulse and premeditation.

Meaning in this sense is the prerogative of actions as contrasted with other experiences. But of course they partake also in the possibility open to the latter of being reflected upon after they have begun to pass away. When an action is completed its original meaning as given in the project will be modified in the light of what has actually been carried out, and it is then open to an indefinite number of reflections which can ascribe meaning to it in the past tense.

So far we have been concerned with the simplest concepts of meaning and action. Before passing to the social sphere it is necessary to consider some further complications relating to these concepts and to the connection between them.

The meaning attributed to an experience varies according to one's whole attitude at the moment of reflection, while one's attitude is also influenced by the nature of the experience reflected on. In particular one varies the degree to which reflection is detailed and explicit or to which past experiences are merely taken for granted, according to one's practical interests.

This holds true not only of single past experiences but of whole complexes of meaning, which are used either explicitly or implicitly as frames of reference in terms of which new experiences are interpreted. The new experiences thus given meaning modify and are assimilated into the previously existing frames of reference. These may also be called complexes of experience (*Erfahrungszusammenhänge*) since they represent the stock of knowledge gained in the course of one's past experience.

The meaning of a particular experience may be regarded as the way in which a place is found for it within a wider complex of meaning. In the case of action the simplest complex of meaning in terms of which it is interpreted by the actor are its motives. Dr. Schütz distinguishes between the in-order-to motive and the because motive of an action. The former refers to the future and is identical with the object or purpose for the realisation of which the action itself is a means. The latter refers to the past and may be called its reason or cause. The project of visiting a friend is the in-order-to motive of my leaving the house. Its because motive may be that he has just rung me up.

We stated previously that the meaning of an action was identical with its project. But this is an over-simplification. For while in the act of projection the project is conceived as a unity, the actor can always in reflection draw a distinction between that part of the action which he is just about to carry out and its in-order-to motive. It would be therefore more correct to say that the project, including the in-order-to motive, is the complex of meaning in terms of which the actor regards the meaning of his action, if for any reason he makes explicit the distinction between them. He also uses explicitly or implicitly his stock of past experiences as a frame of reference, particularly to judge whether the means chosen are appropriate to produce the desired end.

Once the choice has been made between alternative projects and the action itself has been completed, or at any rate begun, the actor may ask himself what has determined him to choose this project rather than another. His answer will give him the because motive. Thus the action is determined by the project, including the in-order-to motive, and the project is determined by the because motive. The complexes of meaning which constitute the in-order-to motive and the because motive respectively differ from one another in that the first is an integral part of the action itself, whereas the latter requires a special act of reflection in the pluperfect tense, which will be carried out only if there are sufficient pragmatic reasons.

To illustrate the main concepts hitherto developed, let us take as an example the project of opening an umbrella in order to keep the rain off. The meaning of the action is the open (or the opened) umbrella, and its in-order-to motive is to keep the rain off. On the other hand the because motive may have been the perception that it has begun to rain, and the relevant complex of experience the knowledge that rain wets one's clothes and that opening an umbrella is a good method of preventing this. If one reflected on this perception without interpreting it as a because motive this would be interpretation of meaning in the general sense explained at the beginning.

3. We now pass to the social sphere. Here again the basic problem is one of meaning and interpretation. As might be expected Dr. Schütz's theory of how one interprets other people's experiences (*Fremdverstehen*) is also closely connected with his notion of time or duration. In this case the fundamental proposition, implied both in social intercourse and in social science, is that there exist more egos than one and that all are constituted according to the same essential laws. In particular every individual's life is a stream of continuous duration, and he reflects on it in the same way as oneself. This "*Generalthese des alter ego*" is, reasonably we think, postulated in this context without further proof, the difficult phenomenological problem what intersubjectivity really is, being left to more purely philosophical thought.

Concentrating on the example of action we have now to ask how one's interpretation of other people's actions differs from one's interpretation, in introspection, of one's own actions.[1] Dr. Schütz's answer to this consists of two points. On the one hand he emphasises that, in contrast to self-analysis, an ego can understand the action of an alter ego while it is actually being carried out by the latter. The process of ascribing meaning is not confined here to the past and to the future perfect tenses, but is possible also in simultaneity.[2] This means that I have experience of other people's minds of a kind which is denied me of my own. I know "immediately" what is going on while it is going on, and I am not restricted to reflections *ex post* and *ex ante*.

On the other hand the alter ego's stream of experience is not directly given to my consciousness like my own stream of experience. On the contrary it is only physical objects, in particular speech, gestures and other movements of the alter ego's body which are directly before me. I interpret these in terms of my whole experience and particularly in terms of similar movements of my own. The chief frame of reference is therefore precisely the connection between such movements and those mental processes of mine to which they habitually give expression. It is only because this frame of reference functions quasi-automatically that

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[1] Anticipating the distinctions drawn up in the following section, it should be noticed that the interpretation of associates (*Umwelt*) is taken as the standard case.

[2] Even essentially actual experiences of the alter ego can be thus interpreted.

I need not take the trouble of explicit reflection and that I can understand the actions of my fellow men by an immediate judgment in simultaneity.

This implies further that my self-knowledge is necessarily more complete than my knowledge of other people, since it is always possible for me in principle to recall previous experiences out of the whole stream of consciousness which I have lived through up to the present, whereas past experiences of the alter ego cannot be drawn upon in this way. My own stream of consciousness is given in its continuity but every other person's " duration " or subjective flow of experience can appear to me only in fragments. However numerous these fragments, they can never constitute for me a continuous flow.

For all these reasons it is impossible that the meaning which one person attaches to the actions of another shall ever be exactly the same as the meaning which the latter attaches to them himself. But the former can, by putting himself in imagination in the actor's place, conceive an in-order-to motive adequate to determine such an action on his part in a similar position. Although therefore the postulate of interpreting the subjective meaning of other people's actions is never completely fulfilled, one can nevertheless approximate to it indefinitely.

It is important to distinguish between *Fremdverstehen* or intersubjective interpretation in the strict sense and self-explication. If I observe someone chopping wood and merely interpret his bodily movements as I should any purely physical occurrence, there is no question of *Fremdverstehen.* Nor is it sufficient to regard his movements as wood chopping, i.e., as movements of a specifically human body. One can only speak of *Fremdverstehen* if my attention is concentrated on the mental processes which constitute for the actor himself the subjective meaning of his action.

Another, somewhat more complicated, example is furnished by the interpretation of *Kundgabehandlungen*—social actions in which symbols (*Zeichen*) are posited for the purpose of conveying information—e.g., a speech. Here again, *Fremdverstehen* does not consist either in hearing words spoken, nor in understanding their meaning (i.e., the objects to which they refer). It consists in understanding on the one hand the mental processes by which the speaker thinks both the words themselves and their meaning in the above sense, and, on the other hand, in understanding the motives which determine him to speak them here and now. Only if one understands in this way the subjective meaning of the speaker, does *Fremdverstehen* take place.

If, on the other hand, one concentrates on the speech itself, one understands only objective meaning, that is to say one interprets the words as symbols belonging to a system of symbols called the English language, and one connects them with the objects which they are normally intended to signify. In the same way, the act of wood chopping already instanced, or a hatchet made for the purpose of chopping wood, has objective meaning, not in the sense that it is entirely independent of human volition, but in the sense that it can be understood without reference either to the way it was made or to the individual who made it. This is the only sense of objective meaning relevant for social science.[1]

4. We are now in a position to analyse the concept of social actions. These are defined as actions whose in-order-to motive contains a reference to someone else's stream of consciousness. They fall into two main classes, for both of which examples have occurred in the previous section. The attempt to interpret other people's actions means that the observer makes the recognition of the alter ego's contents of consciousness the in-order-to motive of his own action.[2] In this case the social action does not imply any attempt to influence the alter ego. If on the other hand his in-order-to motive is to change the alter ego's stream of experiences, we get the second type of social action (*Fremdwirken*). *Kundgabehandlungen* are an example, since they always try at least to make the addressee understand the information imparted. Where the actor tries to provoke an action of the alter ego his in-order-to motive is intended to become the because motive of the latter. This presupposes the subjective chance or expectation that such will be the case.

If social actions take place on both sides, i.e., mutually between two or more persons, or more precisely, if there is an objective chance or probability of such actions, then a social relation exists between these persons. It follows that there are three fundamental types of social relation. Either both partners merely observe one another, or both partners try to influence each other, or thirdly, one of them tries to influence the other while the latter confines himself to observation. If on the other hand one person observes the other without trying to influence him, and also in the knowledge that he is not himself observed by the latter, we have social observation in the strict sense. This does not constitute a social relation.

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[1] We need not, therefore, discuss whether or in what sense real or ideal objects (e.g., mathematics as opposed to the symbols in which it is expressed) have objective meaning independent of Mind altogether.

[2] It should be noticed however that *Fremdverstehen* need not, in the *Umwelt*, be action or even behaviour at all. It may be purely receptive.

Dr. Schütz emphasises that social actions differ substantially according to the social sphere to which they belong. He distinguishes between actions regarding my associates (*Umwelt*), my contemporaries (*Mitwelt*), my predecessors (*Vorwelt*) and my successors (*Folgewelt*).

The mere demarcation of these spheres is fairly simple. My associates are those with whom I actually come in contact. I perceive them directly through my five senses. My contemporaries are those with whom I have only indirect relations. For instance, we carry on a correspondence without actually meeting. But it is always in principle possible for us to meet and, therefore, to become "associates." On the other hand this is not possible with my predecessors who, by definition, belong to the past or with my successors who, also by definition, belong to the future.

It must now be shown why these distinctions are important. The interpretation of associates is characterised by the concreteness and completeness of the symptoms observed, and by their immediate interpretation in simultaneity. If I am conversing with a friend I have not only his words as symbols of objective meaning, but I have also his gestures, the particular modulation of his voice, the expression of his eyes, and innumerable other symptoms which suggest particular interpretations of his stream of consciousness.

In this sphere social observation can usually be changed without difficulty into a social relation, but two differences must be noticed between the one and the other. In the first place I can always interrogate my partner in the social relation if I feel doubtful about my interpretations of his mind, and can thus extend and rectify my knowledge of him. Secondly the close interconnection of our respective because and in-order-to motives allows me to take a good deal for granted about my partner's meaning without further reflection. The observer on the other hand, while he also experiences in simultaneity the constitution of the alter ego's complex of motives, cannot, so long as he remains merely an observer, assume any such connection between his own and the alter ego's consciousness. In both cases interpretation goes on normally in simultaneity, but the process can of course always be interrupted by deliberate acts of reflection.

Of my contemporaries, on the other hand, I have only mediate knowledge. Although my contemporaries, by definition, are living in the same period of (objective) time as myself, this does not mean the simultaneity of our " durations ", as with my associates, but only a certain quasi-simultaneity, since the contents of the alter ego's stream of consciousness, in this sphere, are not directly before my consciousness but form for me only an imaginary duration. It is not their actual bodily movements but only things they have produced that are given to me as symptoms. I cannot draw direct conclusions as to the mental processes expressed in the creation of these products. I can only, in reflection, correlate with the latter typical modes of possible behaviour adequate to explain their creation. The process of interpretation therefore must take the form of constructing ideal types.

Ideal types are abstract concepts. They refer not to an individual or to a number of individuals existing at particular points of space and time, but to an indefinite number of individuals regardless of when and where they exist. They express, moreover, only such qualities as are common to different examples of a general class. In contrast to the interpretation of associates as individuals, my contemporaries are to me always more or less anonymous. The degree of anonymity of an ideal type depends on the extent to which I substitute complexes of objective meaning for the subjective meaning which the interpretation of associates tries to achieve. One must, therefore, distinguish between predominantly personal ideal types and predominantly material ideal types.

It is as a contemporary that we think, for instance, of the post office official. We know approximately what actions are involved in taking our letters to their destination (material ideal type). We then imagine typical mental processes appropriate to these actions and we construct a possible scheme of consciousness of which they are contents. Thus we get the personal ideal type of the post office official, the concrete individual representative of which we may never happen to see.

The two spheres are not rigidly isolated from one another. Thus all one's associates will become contemporaries either permanently or temporarily. Likewise, many, though not all, of one's contemporaries may become associates at and for a certain period of time. Moreover, the demarcation itself must not be understood too rigidly. Here as with all empirical concepts there are borderline cases. Thus on the one hand a conversation by telephone need not be very different from ordinary conversation, although the speakers are distant from one another in space, and on the other hand the spectator at a chess match may concentrate on those aspects of the game which could be interpreted just as well from a printed score.

More important than these borderline cases is the interdependence between the two spheres. The ideal types used in mutual intercourse between contemporaries are ultimately abstractions from immediate experience, especially of one's intercourse with associates. The interpretation of contemporaries is therefore based on the interpretation of associates. On the other hand ideal types, both personal and material, when once constructed are in their turn used as frames of reference in intercourse with associates.

All these strands of thought can be illustrated by the social relation, my friendship with A. Primarily, friendship is a relation between associates. Suppose that A has been conversing with me and then takes his departure. He ceases for a time to be my associate and has become a mere contemporary. From now on till our next meeting I gain no more fresh experience of him in simultaneity. I am dependent on my existing stock of immediate experience and on explicit reasoning from indirect evidence. Unless the latter contradicts the former I assume A to have remained unchanged, or in other words I treat him as a " constant." He has become for me an ideal type. Thus the conception, my friendship with A, means for me firstly the memory of our direct association in the past, secondly the assumption of the interrelatedness of our actions, although not perceived in simultaneity, and thirdly the expectation that we shall resume our relation as associates substantially at the point where it was interrupted.

But the world of contemporaries is not always developed as in this case out of intercourse with one's associates. A's brother B, for example, whom I have never met is for me an ideal type constituted primarily out of the direct account given me by A of his direct relations with B *qua* associate. B is for me necessarily a more anonymous and a less concrete figure among my contemporaries than A, meaning by greater anonymity that I have fewer symptoms and less previous knowledge of B than of A. My interest in him is also less personal and the ideal type which I construct contains therefore less elements inapplicable to other persons.

One can arrange the ideal types used in ordinary life according to their degree of anonymity. The series begun by my absent friend A and continued by his brother B contains as further links Professor C, whose books I have read and whom I am now going to meet personally, and the post office official, whom I still regard as an individual person but who interests me only in so far as he carries out the function of expediting my letters. Then there is the present Parliament, which I know to consist of individuals, but which I regard as a whole. Then abstract social entities like England, which is essentially anonymous; further, the rules of English grammar or the principles of Common Law; and finally material products such as machines, which I regard as having been produced by human beings for the use of human beings, but in complete anonymity. In this series the personal ideal type plays a smaller and smaller part compared with the ideal type representing an objective complex of meaning; by the latter being understood the objective content of actions regardless, not only of what individual carries them out, but what type of person he is and to what group of persons he belongs.

Finally let us take Dr. Schütz's example of how one applies ideal types to an associate. If I observe someone using a screw-driver in a motor works I take for granted on the one hand the objective complex of meaning "construction of motor cars" and on the other hand the personal ideal type of a mechanic receiving wages and working so many hours a day. The number and generality of the types applied varies with my practical interests. Thus I can regard him as a member of the proletariat or more concretely of the Berlin proletariat 1931. If my interest is primarily political, I assume perhaps that he is a social democrat. But unless I see that he is wearing a party badge, or unless I have such knowledge of him *qua* contemporary as that his name is on the list of trade union members, my interpretation is—like all applications of ideal types to particular cases—only a matter of probability. Further experience may show that he is not a workman at all but an engineer, and then all my political conclusions are probably wrong.

The distinction between social relations and social observation applies also to the world of contemporaries, but the technique of interpretation is here, in contrast to the world of associates, in principle the same for the social partner as for the social observer. For there is no direct connection in simultaneity between the motives of the two partners, and interrogation is also possible only indirectly. This means that one's interpretation of symptoms can be rectified only with the help of objective frames of reference which are themselves in principle of doubtful applicability to the interpretation of subjective meaning. Whereas my associate can explain the words he uses in many cases by direct reference to an example, present in our common environment, of the objects referred to, in the world of contemporaries one concept can be explained only in terms of others. It follows that one's frame of reference consists for the most part of objective meanings in the sense of the meaning of symbols.

We may conclude the present section with some remarks about the worlds of predecessors and of successors. The only social action which we can perform with reference to our predecessors is to observe them, i.e., to let them influence us. We cannot influence them and no kind of bilateral action is possible. The observation of one's predecessors differs from the observation of associates or contemporaries, mainly by the fact that the only symptoms one has are on the one hand monuments which they have left behind them and, on the other hand, the direct oral tradition passed down from one generation to the next. Of course there is no possibility of interrogation at all.

Dr. Schütz conceives a thorough analysis of the interpretation of predecessors to be a necessary prolegomenon to any systematic investigation of the historical sciences.

But being concerned in the present book primarily with the social sciences he does not fully elaborate the point. We therefore omit further details.

Finally, as regards the world of our successors the only relevant fact is that we cannot, scientifically, know anything about them. We do, of course, for the purposes of ordinary social life speculatively apply to them ideal types drawn from other spheres, but it should not be forgotten that the future is completely free and therefore beyond the realm of possible recognition.

5. The methodological problems of social science dealt with in Dr. Schütz's concluding chapter all turn on the thesis that the social scientist, *qua* scientist, is an observer of his contemporaries (and of his predecessors). He differs from other observers in this sphere in taking as his frame of reference the whole complex of scientific experience. It is inconsistent with the spirit of the latter to make untrue, implicit or unclear presuppositions, and the social scientist, who uses it, necessarily tries—in contrast to those who observe in ordinary social life—to extend and make more precise this objective complex of meaning and experience. He, too, is not concerned with the individual subjective meaning of actions in the world of associates but with the typical objective and subjective meaning of the social actions of contemporaries. His actions are the rational limiting case of observation in this sphere.

He therefore operates exclusively with ideal types, and it is the aim of every systematic social science to formulate its propositions with the help of a complete pyramid of these concepts. As a starting point the social scientist (at any rate in economics) usually takes the ideal type of rational action—though he does not by any means confine himself to it—because in this case the type of action which he observes has the same structure as his own, and because he can assume that the actor operates with the same complex of experience as he does himself.[1]

The chief methodological problem with which the social sciences are faced is the construction of adequate ideal types. In order to be adequate, ideal types must fulfil two conditions. On the one hand they must satisfy the rules of formal logic. In other words no ideal type or system of ideal types may contain internal contradictions, and when the nature of the problem demands that the ideal type used shall be changed or enlarged, this must be made quite explicit. On the other hand the type selected must be in accordance with the totality of scientific experience, both of a more general and of a more special kind.

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[1] At an earlier stage Dr. Schütz has defined rational action as action with completely explicit ends and means. It should be noted that the ideal type of rational action in the present passage includes the further condition that the objective relation between means and ends is correctly grasped by the actor.

Since the social sciences contain a manifold of heterogeneous problems the ideal types will have to be changed according to the problem in which one happens to be interested. This represents the pragmatic element in interpretation which is present, therefore, not only in ordinary life but also in social science. But once a science, such as economics, has determined in principle which problems are relevant and which problems are irrelevant to it, then it has a criterion for constructing a complete system of adequate ideal types.

Correlative with the concept of adequacy is that of objective chance. The type chosen to interpret a given action is adequate if there is really a good chance that the action has been carried out in accordance with it; if in other words the relevant complex of scientific experience suggests that such a probability exists.

In contrast to objective chance, the concept of subjective chance is, as will be recalled, a category not of observation but of the action observed, being equivalent simply to the expectation that one's action when carried out will correspond to the project which one had in mind. It is relevant here only in so far as the observer must ascribe to his ideal type such expectations as are in accordance with the complex of experience attributed to the latter. The social scientist has therefore to construct ideal types not only in accordance with his scientific experience, but he has also for certain purposes to use the pre-scientific ideal types in terms of which contemporaries interpret one another. Jurisprudence, for example, requires not only an "objective" definition of the legislature but also a conception of how the legislature is regarded by the subject.

The relative importance of personal and of objective ideal types in the social sciences varies, as in the ordinary world of contemporaries, with the degree of anonymity. Thus there may be a history of works of art, or a theory of the state, which ignores the process of creation altogether and concentrates wholly on the product. Pure economics as opposed to economic history or economic sociology uses the completely anonymous ideal type of " one " or " everyone." Since, according to Dr. Schütz, pure economics contemplates the interaction between formalised individuals, he calls this science an objective complex of meaning about subjective complexes of meaning of complete anonymity.

6. Here Dr. Schütz breaks off his analysis and indicates further problems in the same field of enquiry which still remain to be investigated: namely, the problem of sociological personality and the problem of relevance. It is very much to be hoped that he will be able, in some future publication, to deal with them himself.

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In drawing attention to Dr. Schütz's book we may recall, perhaps, that in this country economics has always been pursued as an abstract and a systematic science. Moreover English economists—contrary to a view often held on the Continent—have made very important contributions to methodology. Our knowledge in this sphere is no alien importation but largely the product of J. S. Mill, Senior, Cairnes, Mr. J. M. Keynes, Professor Robbins, etc.

The controversy started by *The Nature and Significance of Economic Science* shows that interest in methodological problems is by no means dead in the Anglo-Saxon countries. It therefore seems opportune to place the subject in a wider setting since the range of problems dealt with has usually been somewhat narrow.

Of course philosophy and general sociology can never be a substitute for economic enquiry. Indeed their usefulness to the economist as such is purely negative. They can only make explicit the implications of his fundamental concepts and thus guard him from misunderstanding his problems and misinterpreting his results.

In this sense Dr. Schütz's analysis of the distinction between social and non-social activity is clearly relevant to the problem whether there are significant differences between economic activity in the ordinary sense of buying and selling, and the pure acts of preference represented in the modern theory of value by indifference curves. Similarly the theory of ideal types is relevant to the problem whether economic theory, of whatever kind, is adequate to explain concrete facts. Further the now fashionable distinction between *ex ante* and *ex post* involves Dr. Schütz's problem of thought in the past and in the future perfect tense while the concept of expectations[1] can be given greater precision in terms of subjective as opposed to objective chance. There is no imperative need for the economist to bother about these problems any more than about the intricacies of mathematics or epistemology. But knowledge is one, and he neglects them at his peril.

For reasons of space no attempt is made here to work out these connections, particularly as it is hoped that Dr. Schütz will expound in this Journal the application of his theory to economics.

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[1] To the relevance of which our attention was drawn by Dr. Lachmann.

Some Aspects of Discrimination by Public Utilities

By C. L. Paine

One of the most important features of that group of undertakings to which the term Public Utilities is applied is that it produces for the most part under conditions of decreasing average cost. Since under these conditions marginal cost is necessarily less than average cost, it follows that the average price received by such an undertaking must be greater than its marginal cost if it is to cover its total costs. It is this fact which explains the search which is continually being made by Public Utilities for new markets which yield a lower price than the average received by the undertaking as a whole, but higher than the marginal costs of the extra output sold.[1] One of the most important devices for separating out such markets is the system of charging known variously as the "All-in Rate," and the "Two Part Tariff." The use of this system has extended rapidly in the last few years, and even since the first draft of this paper was written the Gas Light and Coke Company has announced an important new tariff of a similar kind. In what follows an attempt will be made to examine the general economic consequences of the adoption of the system by Public Utility Undertakings.

The normal method of charging is to make a fixed charge not varying with consumption, and to charge actual consumption at considerably less than the standard rate for ordinary consumers. Sometimes, however, consumption up to a predetermined amount is charged at one price, and any consumption in excess of this at a lower rate. But as we shall see both these methods yield substantially the same result. If the two part tariff were the only method of charging the undertaking would maximise its profits by charging, in addition to a price equal to marginal cost, the maximum fixed amount which would still retain the custom of each consumer. This would in fact amount to perfect discrimination.[1] But quite apart from the effect of such a policy on public opinion, the power of the monopolist would be limited by yet another fact. Since he would be unable to calculate the maximum fixed charge accurately for each person, and in particular if the fixed charge were based on some such arbitrary characteristic of the consumer as the floor area of his house or its rateable value, he would undoubtedly make the fixed charge too high for some consumers and thus lose their custom altogether. In order to trap these consumers the monopolist would quite probably find it profitable to continue a single price tariff of the ordinary kind. In any case whether for this reason, or because a public body controls or threatens to control rates, the writer knows of no case in which a two part tariff has been adopted and where the consumers have not at the same time been given the option of continuing under the old flat system of charging.

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[1] I am not suggesting that Public Utilities will not also be looking for markets yielding higher than their average receipts, but this is natural and general and calls for no special explanation.

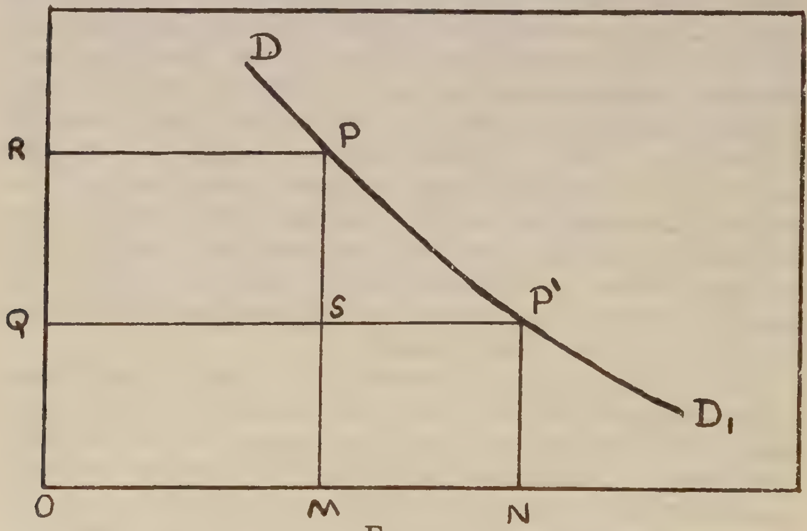

FIG. 1

The fact that the consumer thus has a choice between two entirely different systems of charging has important consequences. For the level to which a Public Utility Undertaking can raise its fixed charge to any individual consumer is limited by the fact that, if the consumer considers the extra consumption which he enjoys under the two part tariff is not worth the increase in his payments to the undertaking, he will return to the old system of charging. Thus in Fig. 1 DD_1 represents the demand curve of an individual consumer, the flat price currently being charged is MP, while the consumer buys at this price an amount OM. Let us further suppose that NP' represents the price which the undertaking charges to those consumers who buy under a two part tariff. Now the fact that if our consumer buys at a price NP' he has to pay a fixed charge may affect his consumption at that price. But we may neglect this fact for the moment and assume that at the lower price he will consume an amount ON, which is the same as he would consume at that price even if the fixed charge were waived altogether. We may now enquire to what extent the undertaking can raise the fixed charge without inducing the consumer to change over to the alternative system.

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[1] This, as far as I have been able to understand it, is the subject of an article in Polish by A. Wakas, in the *Ekonomista*, 1935, Vol. 1, which has been very kindly translated by Dr. A. M. Neuman. As, however, I have had no opportunity of studying a written translation I refrain from any detailed reference to this article.

The extra consumption which the consumer obtains under the two part tariff is the amount MN. Now although the extra sum which the consumer would be willing to pay for the amount MN is only equal to the area under the demand curve $PP'MN$ under very peculiar conditions, at least we know that it cannot be greater than this amount. We know therefore that the undertaking cannot make the fixed charge greater than the area $RPP'Q$ without causing the consumer to change over to the flat system of charging. If, on the other hand, the undertaking were to allow the consumer to take up to OM at the price MP, and any further amount at the price NP', its total sales would be exactly the same as before but its receipts would be equal to the area $ORPSP'N$. These two systems of charging which we have just considered therefore yield substantially the same results, and when we also take into account the fact that the maximum amount extra which the consumer would be willing to pay for his increased consumption is almost certainly less than $NMPP'$, and that in any case the undertaking has no means of gauging with any degree of accuracy the nature of each consumer's demand curve, it will be seen that for all practical purposes the two systems are synonymous. In what follows therefore we shall analyse the system of two prices and assume the other yields substantially the same results.

The system we are analysing is then as follows. Having fixed its two prices the undertaking will determine for each consumer the amount which he would consume at the higher price if this were the only price ; any consumption up to this amount will be charged at the higher price, while any additional consumption will be charged at the lower rate. Any buyer whose consumption remained unchanged, therefore, would pay no more than before, but as we have already pointed out it would in fact pay him to expand up to the point where the marginal significance of the service in terms of money (in the case of a final consumer), or the value of its marginal product (in the case of a producer) is just equal to the marginal cost of the service, i.e., the lower price. The values of the marginal products of the factors transferred to the undertaking in order to produce the increase in output rendered profitable by the adoption of the two part tariff will thus be their physical marginal products multiplied by the lower price. But is not the value of the marginal products of the extra factors in their alternative uses represented by the marginal cost of the undertaking, which shows the price which it has to pay for the extra factors necessary to increase output by one unit ? And since the undertaking will always find it unprofitable to fix its lower price below marginal cost, can we not therefore say that the adoption of a two part tariff must necessarily bring the actual distribution of resources nearer to that " ideal " which would result from perfect competition ?

But these propositions do not necessarily follow. For the prices which the undertaking pays for its factors do not represent the values of their marginal products in alternative uses if the undertaking draws them from other Public Utilities which are similarly working under conditions of decreasing cost. Only in the case where the factors are drawn away from an industry working under conditions of perfect competition can it be said unequivocally that the new distribution brings the values of marginal net products nearer to equality in all uses. Even if all the undertakings which are in competition with one another for factors were to adopt the system, it would still not be possible to say that the new distribution must be nearer the "ideal", unless the supply of factors to the industry were fairly elastic, and the industries from which they were drawn were perfectly competitive. For the elasticities of the demand curves of the various undertakings will vary considerably, and the extent to which it pays them to expand their demands for factors as a result of the adoption of the two part tariff will therefore also vary. Consequently the degree of divergence between the values of the marginal products of the factors employed by the various undertakings may increase. Nevertheless it may still be possible to say that the new distribution is nearer to that which would be produced by perfect competition, if the output of every undertaking is increased. For, in this case, though the increase in output will have been uneven, marginal costs will have been brought nearer to price in every case. But if the supply of factors to our Public Utility Undertakings is inelastic the general adoption of a two part tariff might so raise the prices of the factors that those undertakings, whose increase in output would in any case have been small, may find it profitable, at the new level of costs, actually to decrease output. Though therefore there will have been an increase in the total quantities of factors employed by all undertakings, in particular cases there will have been decreases, and there is no economic calculus which will enable us to determine whether the new distribution is nearer to that which would equalise the values of the marginal products of every factor in all uses.

This result is subject to some qualification, however, in one case. So far we have assumed that the undertaking is trying to maximise monopoly profits. If it is publicly owned or controlled, however, it may possibly try to sell the largest output consistent with equality between total costs and total receipts. If such an undertaking were to adopt a two part tariff it would set the upper price at the level of its average costs, and the lower price at the level of marginal costs. Now if a number of similar undertakings were to adopt two part tariffs, the arrangement could result in no diversion of resources, as between the undertakings, such as we considered above. For in every case the lower price would be equal to marginal cost, and consequently we may say that if the group competes for its factors exclusively with perfectly competitive industries, the new distribution must be nearer to that which would equalise marginal value productivity in all directions. No such conclusions can be drawn in the case of a single such undertaking, however, nor in the case of a group of undertakings drawing its factors from other undertakings interested in maximising monopoly profits, or from imperfectly competitive industries.

Before leaving this question of the distribution of resources we may perhaps compare the results of a two part tariff, such as we have been considering, with those that would be achieved by subsidising the undertaking so as to enable it to sell at a price equal to marginal cost. The " maldistribution " of resources which it is thus proposed to remedy, results from the fact that some units of monetary demand are exerting more than the pull on the factors of production which they would have in conditions of perfect competition, while others are exerting less than this pull. Now if the subsidy to the latter can be raised by taxing the former, it can be shown that the resulting distribution of resources approaches more closely to that which would equalise marginal value productivity. But if the subsidy is raised out of general taxation, demands quite different from those which are exerting more than their proper pull on the factors will be cut off, and we have no means of assessing the relative importance of these demands and of the demands for the services of the Utility Undertaking. Moreover, a subsidy to all undertakings producing under conditions of decreasing cost conditions, sufficient to enable them to charge a price equal to marginal cost, would enable undertakings to come into existence which could not cover total cost even under conditions of perfect discrimination. It is true that an individual buying a piece of fixed equipment will use it up to the point where the marginal significance in terms of money of the service it provides is just equal to the marginal cost. But he will not instal the equipment unless he considers that the return he gets is sufficient to cover not only prime costs but also fixed costs. Similarly there is nothing in the ideology of perfect competition which would justify the installation of a fixed plant unless the consumers of its services are willing to pay not only the opportunity costs of the marginal increment of factors, but also the opportunity costs of the whole block of resources comprising it.

A properly designed two part tariff suffers from neither of these defects. Once the desirability of increasing output is conceded, it can be shown that factors are diverted in the right directions, while no undertaking can stay in existence in the long run if it cannot cover its total costs. Moreover these tariffs have advantages over other forms of discrimination which also increase output in that they enable the consumer to increase his consumption in every use instead of only in the uses for which the price is lowered.

II

So far we have not considered in any great detail the factors determining the prices at which the monopolist will maximise his profits. The amount which the monopolist can expect to sell at the lower price will, as we have seen, be determined partly by the amount which he has already sold at the higher price. We have to determine therefore the optimum discriminatory prices in two markets whose demand curves are interdependent. This problem is somewhat complicated and we shall begin with a somewhat simplified case where there are no costs of production (or what comes to the same thing where costs are constant), and where the demand curve is a straight line.

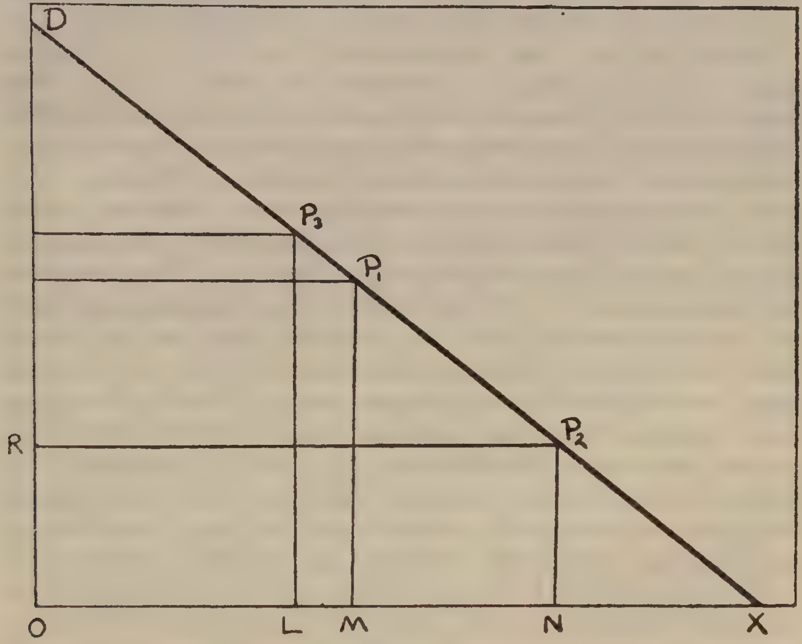

FIG. 2

In Fig. 2 DX is the demand curve of the monopolist and OX is the output at which price becomes zero (or equal to marginal cost). Then if the monopolist sells at only one price he will sell an output OM (equal to half OX) at a price MP_1. Now suppose he decides to sell additional units at a lower price. It is probable that at the lower price there will be consumers who would not have consumed at all at the higher price, and since, under a two part tariff, no consumer will be allowed to buy at the lower price unless he has bought his agreed quota at the higher price, these potential buyers will not in fact become consumers. Moreover, as we have already seen, even those consumers who do buy at the higher price may buy less at the lower price than if that were the only price. For both these reasons the curve which shows total sales at two prices may be expected to lie to the left of DX which shows sales at a single price. For the moment, however, we propose to neglect this fact.

If, therefore, the monopolist fixes his lower price at NP_2 the total amount which he may expect to sell is ON, of which OM will be sold at the price MP_1, and MN at the price NP_2. In fact if MP_1 is regarded as the new Y axis P_1X becomes the demand curve at the lower price.

Given the fact that the monopolist is charging a higher price of MP_1, in order to maximise his profits at the lower price, NP_2 will have to be fixed such that $MN = \frac{1}{2} MX$. But the fact that he is now selling at the price NP_2 completely changes the higher price which is most profitable. For given our present assumptions the total amount which can be sold depends solely on the lower price, and is completely independent of the higher price. This being so any reduction in the amount sold at the higher price must be compensated by an increase in sales at the lower price. It follows that the rectangle RP_2NO is a constant item in receipts which is independent of the higher price, total receipts being just reduced to this amount when the higher price exactly coincides with the lower price. In fixing the higher price therefore the object of the monopolist will be to maximise the area of the rectangle inscribed in the triangle DP_2R. He will do this by fixing a price LP_3 such that $OL = \frac{1}{2} ON$. But this change in the upper price will have reactions on the demand curve at the lower price which will now become P_3X, and it will now obviously pay to sell at this price an amount equal to $\frac{1}{2} LX$. The final position of equilibrium will be reached when the amount sold at each price is equal to one third of OX. This position is shown in Fig. 3. The upper price is MP_1 and the lower price NP_2. No further gain could be made by adjusting either price since $OM = \frac{1}{2} ON$, while $MN = \frac{1}{2} MX$.

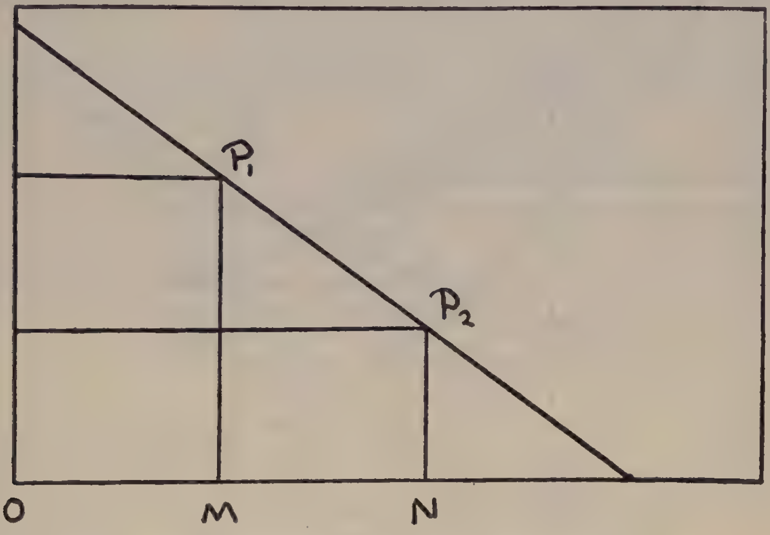

FIG. 3

We may now pass on to consider the case of rising marginal costs. The case of increasing cost does not differ appreciably from the case we have already analysed. In Fig. 4 DD_1, DR_1 and MC are respectively the simple demand curve, the marginal revenue curve, and the marginal cost curve. OB is the simple monopoly output and BP_1 the price. If OB, the amount already sold at the higher price, be deducted from the demand curve and BP_1 be thus regarded as the new Y axis, P_1D_1 becomes the demand curve at the lower price. To this demand curve a marginal revenue curve P_1R_2 can be drawn. It will now pay the monopolist to sell an amount BC at a price CP_2 in addition to the amount he is already selling at the higher price, since at this lower price the marginal revenue curve P_1R_2 cuts the marginal cost curve MC. But as we have already seen, so long as the monopolist sells at a lower price of CP_2, the area $COTP_2$ becomes a constant item in his receipts, so that his object will now be to set the upper price at the point which maximises

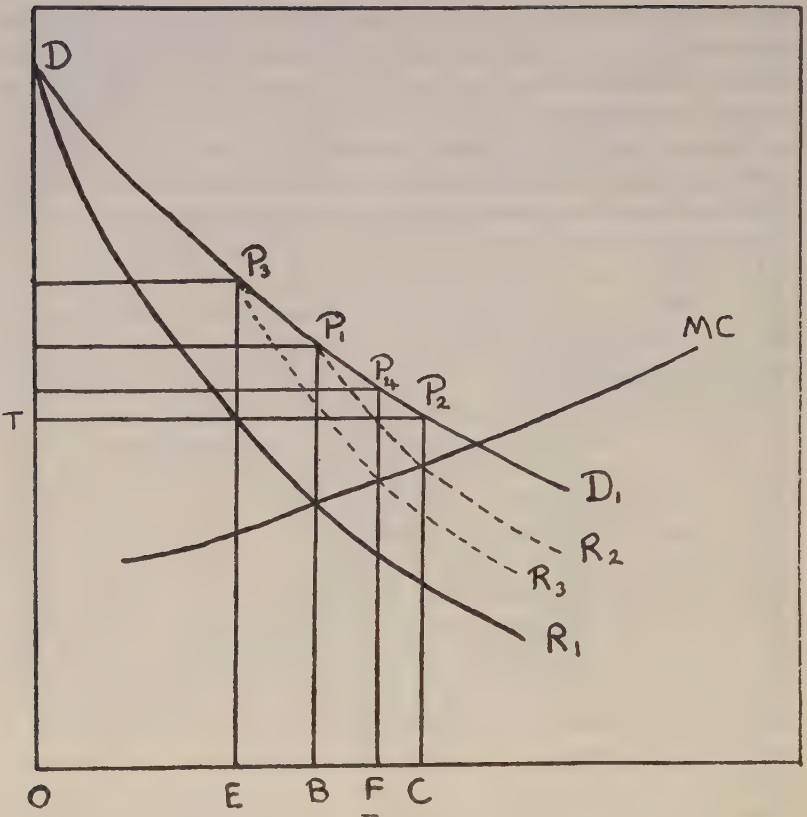

FIG. 4

the area inscribed between the demand curve DP_1 and the horizontal line TP_2. This he will obviously do by fixing his upper price at EP_3 since at this price the marginal revenue curve cuts TP_2. But this rise in the upper price will change the most profitable lower price, since P_1D_1 will no longer be the demand curve at the lower price. It will in fact now be P_3D_1 and the new marginal revenue curve to it will be P_3R_3, while the new lower price will be FP_4, this being the price at which P_3R_3 cuts MC. This process will go on, each successive price rise being smaller than the one preceding it until it no longer pays to raise either price.

The amount by which it pays to raise the higher price above the simple monopoly price will be greater by reason of the rising marginal costs. This can easily be shown in Fig. 5. BP_1 is the simple monopoly price and OB the output.

If marginal costs are rising as in MC_1, it will pay to sell an amount BC at the lower price CP_2, marginal revenue in the lower market (P_1R_2) here being equal to marginal cost. It will now pay to raise the upper price so as to maximise the area of the rectangle lying between the demand curve and P_2T, and this will be done by fixing it at EP_3, the price at which the marginal revenue curve DR_1 cuts P_2T. If costs had been constant, however, as in MC_2, then P_1R_2 would have cut the marginal cost curve to the right of its intersection with MC_1. The original lower price FP_4 would therefore have been lower than CP_2, and consequently the initial increase in the upper price would also have been less. In a similar manner it can be shown that the final equilibrium prices will also be lower in the case of constant than in the case of rising marginal costs. Concavity of the demand curve would

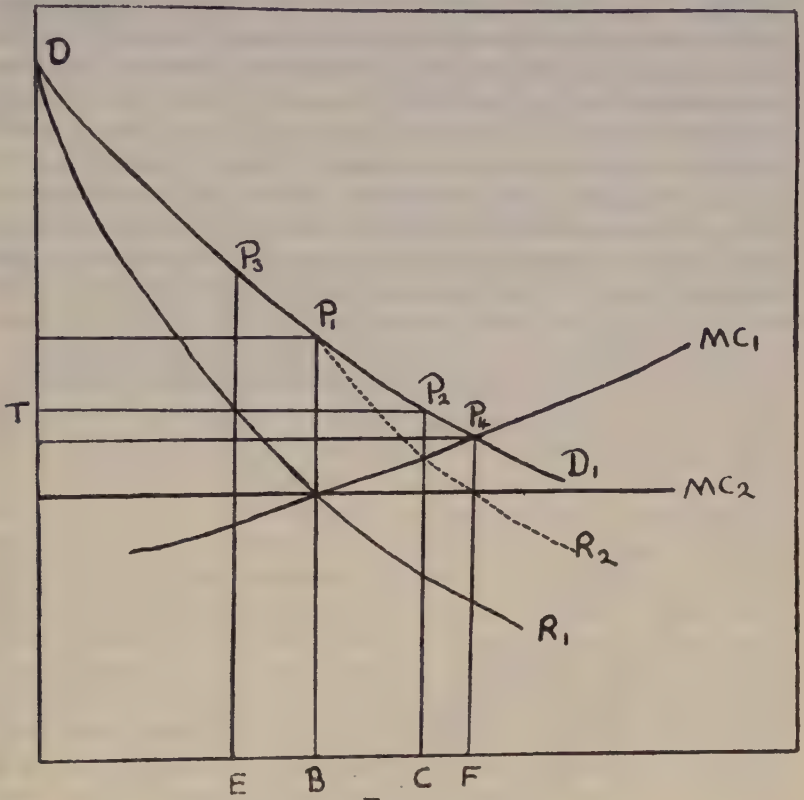

FIG. 5

also appear to have a similar effect in raising the upper price, since if the demand curve is inelastic in its higher reaches (as it almost certainly is in the case of electricity) it will clearly pay to raise the price more than if it were elastic. On the other hand increasing elasticity in the lower reaches of the curve will have the effect of reducing the lower price.

As might be expected the effect of falling marginal costs is to lower prices below what they would be under constant costs. Given extremely rapidly falling marginal costs, it might pay to reduce the higher price even below the simple monopoly price. Such a case is worked out in Fig. 6. As before, DD_1 is the simple demand curve, DR_1 is the marginal revenue curve, MC is the falling marginal cost curve, while OB is the simple monopoly output and BP_1 its price.

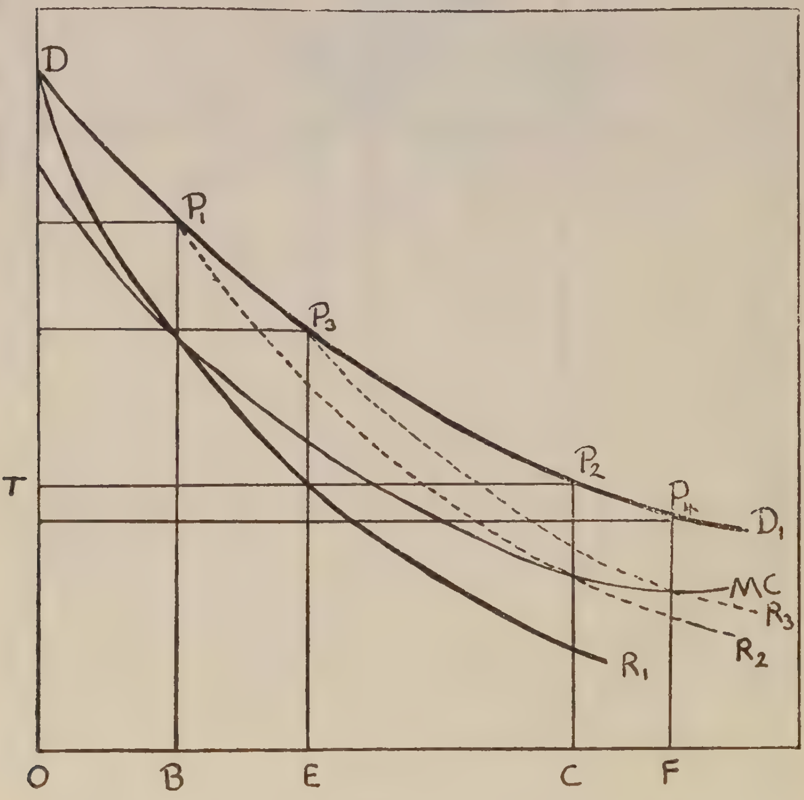

FIG. 6

P_1R_2 is the marginal revenue curve for the lower market, the most profitable amount to sell there being BC at a price CP_2. It will now pay to reduce the upper price to EP_3, the price at which the marginal revenue curve DR_1 cuts P_2T. But this reduction in the upper price will reduce sales at the lower price. It will now pay to reduce the lower price to FP_4, the point where P_3R_3, the new marginal revenue curve for the lower market, cuts the marginal cost curve. Thus these successive price changes will go on in the way we have already analysed above, each reduction being smaller than the one which preceded it, until the limit is reached where it no longer pays to alter either price.

It is clear that this case which we have just been examining is a most improbable one. It would only pay to lower the upper price below the simple monopoly price in conditions of extremely rapidly falling costs. Since average cost continues to fall for some time after marginal cost has begun to rise, such rapidly falling marginal costs could only occur if the plant were very small in relation to the size which gives minimum production costs. They are only likely to occur in fact if the size of each undertaking were limited to a municipal or administrative area—and surely even in this case there would be room for some sort of Central Electricity Board and Grid Scheme—or if transport costs were prohibitively high. In most cases that are likely to occur in practice therefore, the effect of falling marginal costs would seem to be merely to retard the rise in the upper price, and not actually to lower it.

It has already been pointed out that if the demand schedules of all consumers are not of a very similar nature, there will be potential buyers who would consume at the lower price but who would not be prepared to buy at all at the upper price, and that as a consequence of this we must deduct from the demand curve at the lower price the amount that would have been consumed by this class of consumers if they had been allowed to take the whole of their consumption at the more favourable rate. So far we have neglected this fact but it is now necessary to take account of it. The higher the upper price the greater will be the number of potential consumers whose custom is lost in the way we have described. The total amount sold which under our previous assumptions was dependent only on the lower price, now becomes partially dependent on the upper price, and will decrease somewhat with every increase in the latter. When we take into account, therefore, the repercussions of a rise in the upper price on total sales, it becomes clear that the profitability of raising the upper price is slightly less than that shown by our previous analysis. On the other hand since under this method of charging any reduction in the lower price can result in increased sales only to *existing customers* (i.e., individuals who are prepared to consume something at the higher price) and not to entirely new customers, the lower demand curve will be less elastic than it otherwise would be, and it will therefore be less profitable to reduce the lower price.

We have thus seen that, subject to certain reservations with respect to the case of falling marginal costs, there is an overwhelming probability that it will pay to raise the upper price above the simple monopoly price, but that the lower price is likely to be considerably below the latter. It will of course be necessary to raise the " flat " price under the alternative single price system to somewhere very near the upper price of the two part tariff, since otherwise it might not be possible to induce consumers to change over to the more profitable system. As a result of the new form of discrimination then, the receipts of the undertaking from each consumer will be greater, but, on the other hand, since the lower price will be below the simple monopoly price, the consumption of each individual will be nearer to the optimum, this being defined as the amount he would consume at a flat price equal to marginal cost. Can we therefore say that, given the desirability of increasing the output of the undertaking—and this depends on the considerations set out in the first part of this paper—the two part tariff, even in the hands of the most rapacious monopolist, results in a distribution of resources superior to that achieved by simple monopoly ?

But any such general conclusion would surely be fallacious. Any individual consumer will find that he has an increased supply of the service provided by the Public Utility Undertaking, but a decreased supply of the commodities on which he has economised in order to meet the increased bills of the undertaking. He may or may not regard this position as superior to the old one. If, as seems probable, the undertaking has a concave demand[1] curve highly inelastic in its upper reaches, it seems probable that the rise in the upper price above the simple monopoly level will be sufficiently great to make most consumers regret the new system, unless the marginal costs of the undertaking are falling very rapidly. It follows from this that perfect discrimination must produce a distribution of resources inferior to that achieved by simple monopoly and *a fortiori* to that achieved by perfect competition. For the position of any individual would be unchanged if he were to increase his payments to the undertaking by exactly that amount which he would be willing to give for the increase in his consumption. But under perfect discrimination he would be forced to pay not only this amount but also the maximum amount he would be willing to pay rather than go without the whole of his consumption of the product.

We have seen that under the most probable conditions of demand and cost—rising or constant marginal costs and concave demand curve—the two part tariff as used by a profit seeking monopoly is likely to lead to an inferior distribution of resources. This result is of course subject to qualification however in the case of falling marginal costs.

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[1] It is perhaps worth pointing out that discrimination only becomes really profitable when the demand curve is concave. For convexity of the demand curve would mean either that one of the markets was unimportant, or that the elastic market rapidly becomes inelastic and the inelastic market rapidly becomes elastic.

The Principle of Increasing Risk

By M. Kalecki

The subject of this paper is the determination of the size of investment undertaken in a certain period by a given entrepreneur. He intends, for instance, to build a factory producing a certain product. He is faced with given market conditions : he knows the price of the product in question, the level of wages and of prices of raw materials, the cost of construction and the rate of interest. Besides he has some rather vague ideas as to the probable future change of prices and costs. This knowledge is the basis for the planning of investment, i.e., for the choice of the amount of capital k (measured in terms of money) to be invested and the method of production to be applied.

With a *given* amount of capital k and a given method of production the entrepreneur is able to estimate the series of future returns (differences between revenues and effective costs) $q_1, q_2, \ldots\ldots\ldots q_n$ during the prospective life of the factory. We shall call the rate ϵ at which the series of returns must be discounted in order to obtain the amount invested k—the efficiency of investment,[1] whilst by prospective profit p we denote the product $k.\epsilon$. Now we can assume that with given amount invested k the entrepreneur chooses such a method of production as would maximise the efficiency of investment or what amounts to the same (k being given) the prospective profit $p=k\epsilon$. Thus to every value of k there corresponds a definite value of maximum prospective profit p_m:

$$p_m=f(k).$$

Now the method of production being chosen for each value of k the entrepreneur has still to define the optimum k, i.e., the size of investment. He must charge the capital invested at the market rate of interest and make also some allowance for risk whose rate we denote by σ. Thus the entrepreneur's prospective gain g is:

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[1] This definition is identical with that of Keynes' marginal efficiency of an asset.

$$g = p_m - (\rho + \sigma) k.$$

The entrepreneur will obtain the maximum gain at the value of k which satisfies the equation

$$\frac{dp_m}{dk} = \rho + \sigma$$

and this value of k is the optimum amount to be invested. Now $\frac{dp_m}{dk} = f'(k)$ is nothing else than the efficiency of small capital addition dk to the amount invested k – supposing that both k and $k+dk$ are invested with optimum method of production. We shall call $f'(k)$ marginal efficiency of investment. We can consequently say that the size of investment k_0 is determined by that level at which marginal efficiency of investment is equal to the sum of the rate of interest ρ and rate of risk σ.

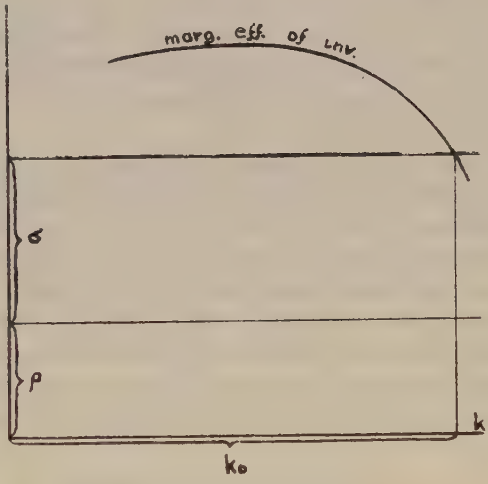

FIG. 1

As can be easily seen from the chart the optimum amount k_0 to be invested is finite only if marginal efficiency of investment falls when k exceeds a certain value. This fall may be caused by: (1) large scale dis-economies; (2) imperfect competition. The first reason though generally admitted seems to be not very realistic. It is quite clear that its technological interpretation is wrong: it is right that every machine has an optimum size but why not let ten (or more) machines of given type work? Thus only the argument of difficulties of management arising out of the large scale of enterprise remains. But also that is doubtful (why not start ten factories instead of one with ten independent directors ?) and at any rate concerns only such industrial giants as are far above the average size of existing enterprises.

The explanation of the limited size of investment by imperfect competition is more realistic but also does not cover the ground. The imperfection is often slight and in that case immense enterprises would be planned.[1] Further the imperfect competition cannot explain the fact that in a given industry at the same time large and small enterprises are started. Thus there must exist a factor restricting the size of investment which we have not taken into account in the above argument.

II

We have assumed till now—as is usually done—that the rate of risk is independent of the amount invested k. And it is this assumption which has to be dropped, I think, in order to obtain a realistic solution of the problem of "limited investment."[2]

There are two reasons for the increase of marginal risk with the amount invested. The first is the fact that the greater is the investment of an entrepreneur the more is his wealth position endangered in the event of unsuccessful business.

The second reason making the marginal risk rise with the size of investment is the danger of "illiquidity." The sudden sale of so specific a good as a factory is almost always connected with losses. Thus the amount invested k must be considered as a fully illiquid asset in the case of sudden need for "capital." In that situation the entrepreneur who has invested in equipment his reserves (cash, deposits, securities) and taken "too much credit" is obliged to borrow at a rate of interest which is higher than the market one.

If, however, the entrepreneur is not cautious in his investment activity it is the creditor who imposes on his calculation the burden of increasing risk charging the successive portions of credits above a certain amount with rising rate of interest.[3]

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[1] If the competition is perfect and the marginal efficiency is greater than the sum of rate of interest and risk, k_0 is infinite (see Fig. 1).

[2] The principle of increasing risk was already used in my article "A Theory of the Business Cycle," *Review of Economic Studies*, February, 1937, pp. 84–85.

[3] See M. Breit, "Ein Beitrag zur Theorie des Geld- und Kapitalmarktes," *Zeitschrift fuer Nationaloekonomie*, Band VI, Heft 5, p. 641.

The amount invested k_0 is now given by the condition that the marginal efficiency of investment is equal to the sum of marginal risk σ and the rate of interest ρ. The $\rho + \sigma$ curve is not a horizontal curve as in Fig. 1 *but an upward sloping one.* The point of intersection with the marginal efficiency curve determines the size of investment and this point exists even in the absence of large scale diseconomy and imperfect competition (Fig. 2).

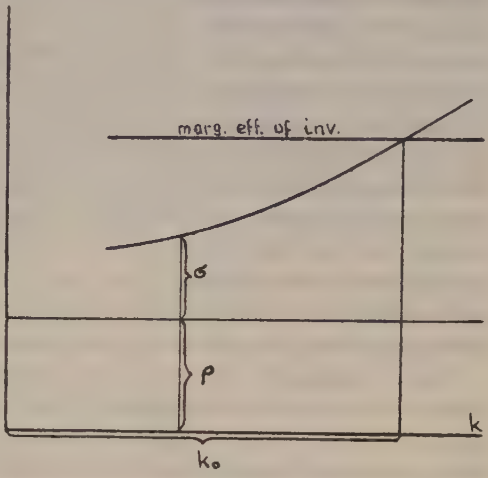

FIG. 2

Now the various sizes of enterprises started in the same industry at a given moment can be easily explained. The smaller is the own capital of an entrepreneur investing the amount k the greater the risk he incurs. For his possible losses bear a greater proportion to his wealth and—since the amount of credits considered by his creditors as "normal" is in a certain proportion to his own capital—the danger of "illiquidity" is greater too.

Thus the smaller is the own capital the higher lies the $\rho + \sigma$ curve and—as is easy to see on the chart—the smaller is the amount invested k_0. The enterprises started in a given industry at a given moment are not of equal size because the own capital of entrepreneurs is not equal. The "business democracy" is a fallacy: the own capital is a "factor of investment."

III

In the case represented in Fig. 2 we have constant returns to the scale (the imperfect competition is neglected) and thus constant marginal efficiency of investment. Thus the marginal efficiency curve represents a constant method of production.

Let us now consider what happens if the rate of interest is lowered. The $\rho + \sigma$ curve shifts down and its point of intersection with the marginal efficiency curve moves to the right (Fig. 3). The method of production applied by the entrepreneur in his plan does not change whilst the size of investment increases. *Consequently so long as we can consider the influence of imperfect competition as negligible and thus have constant returns to the scale the change in the rate of interest does not affect the method of production admitted by the entrepreneur in his plans but only the size of investment planned.*

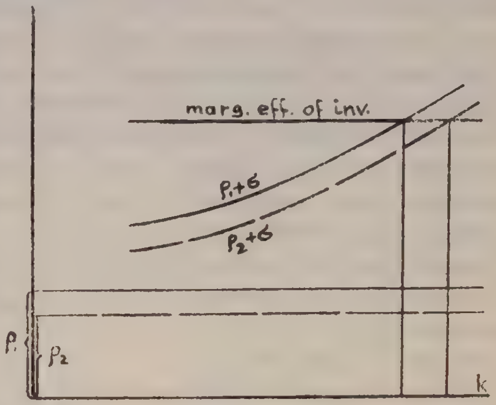

Fig. 3

This statement seems to be in contradiction with the classical theory of marginal productivity of capital and labour but the contradiction is only apparent. The point of departure of the classical doctrine is a drastically simplified model of production in which the quantity of product is a definite function of the amount of "real" capital and labour used. The necessary condition of equilibrium is the equality between the marginal productivity of each factor and its price divided by the price of product. In the case of constant returns to the scale (homogeneous production function) this is also a sufficient condition for long run equilibrium, for with constant returns the marginal productivities' equations exclude the existence of the entrepreneur's gain. If the rate of interest falls, a shift towards a new long run equilibrium must take place in which the marginal productivity of capital is lower, that of labour higher, and more capital is used in proportion to labour. But the theory says nothing about the immediate influence of the fall of the rate of interest on the *plans* of the entrepreneur. Such a fall in the case of constant returns to the scale (and if our principle of increasing risk is not taken into account) must create a tendency to plan investment of infinite size and with indefinite method of production (every method applied on infinite scale gives an infinite profit). Only after the output of the product is suddenly increased its price falls and this makes again possible the restoration of long run equilibrium in which the marginal productivity equations are satisfied.[1]

Thus our problem was quite different from that of classical theory. We examined the planning of the entrepreneur in a given situation which in general is *not* the position of long run equilibrium. We tried to find the factor limiting the size of the investment planned which, as we see, is non-existent in the classical theory in the case of industry subject to constant returns to the scale and not in the position of long run equilibrium. And also our statement that the fall of the rate of interest does not affect the method of production but only the scale of investment plans referred only to the *plans* but not to the situation arising out of their realisation.

The classical thesis of the low rate of interest causing the use of more capitalistic method of production was often applied not only for long run equilibrium position but also for entrepreneurs' planning in "disequilibrium." This interpretation is of course wrong and as we have shown above, if we neglect imperfect competition and introduce the principle of increasing risk the fall of the rate of interest has no immediate effect on the method of production applied in the plans of entrepreneurs.

IV

One intricate point is still involved in the matter above examined. We mean the problem of the *rate* of investment decisions per unit of time. But before dealing with it we shall transform the Fig. 2 a little. We subtract from the ordinates of both marginal efficiency curve and $\rho + \sigma$ curve the rate of interest ρ and thus obtain the marginal net profitability (marginal efficiency minus rate of interest) curve and the marginal

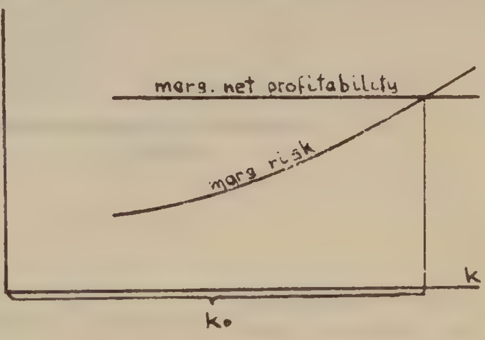

FIG. 4

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[1] This treatment is however quite formal, for in reality cumulative process arises which causes (if the rate of interest is not raised) either hyperinflation or fluctuations about the new position of long run equilibrium.

risk curve. The point of intersection of these two curves gives of course the same value k_0 as above.

Let us divide the time into equal periods $\triangle t$ short enough to consider the economic situation, i.e., the level of prices, wages, etc., as constant within the period (the change of this situation is thus to be imagined as concentrated at the beginning of the period). In each period the entrepreneur takes so much of investment decisions as to equate at the end of it the marginal risk to the difference between the marginal efficiency and the rate of interest. Thus it would seem that no investment decision at all will be undertaken in a period if the " economic situation " remains the same as in the preceding one, because the point of intersection of the marginal risk curve and the marginal net profitability curve lies on the ordinate axis at the beginning of the period considered. This is, however, wrong. For one thing does change in the position of our entrepreneur: during the period $\triangle t$ he saves in general a certain amount s. $\triangle t$ out of his income.

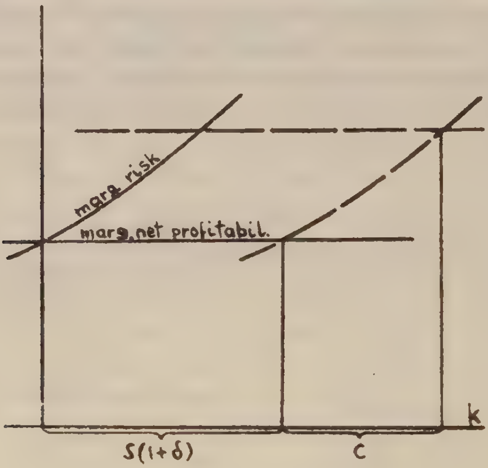

FIG. 5

This accumulation of savings causes a parallel shift of the curve of marginal risk to the right. For the entrepreneur can invest the new accumulated amount without reducing his safety or increasing illiquidity. He may invest even more : if the relation of his net indebtedness (difference between debts and claims) to his own capital is δ, he can freely invest $s(1+\delta)\ \Delta t$.[1]

Such is then the amount of investment decisions if the situation in the period examined is the same as in the preceding one. If, however, this is not the case and the net profitability curve has shifted, also an amount c due to the change of the economic situation will be invested (see the chart). The investment planned during the period Δt consequently amounts to $s(1+\delta)\,\Delta t + c$ and thus the rate of investment decisions during the period is:

$$d = s(1+\delta) + \frac{c}{\Delta t}.$$

$\frac{c}{\Delta t}$ depends on the velocity of change of marginal net profitability. We denote it by v and thus have:

$$d = s(1+\delta) + v.$$

This equation means that *the rate of investment decisions of a single entrepreneur depends on his capital accumulation and on the velocity of change of marginal net profitability.*

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[1] It is now clear that the marginal risk + rate of interest curve dealt with in the preceding paragraphs must be taken in the position at the end of the period in which investment activity was considered.

Parenthood and Poverty

By H. S. Booker

Sir John Orr, investigating the state of nutrition[1] of the people, considered everyone, adult or child, as being of equal significance. Thus he ignores the customary division which considers the requirements of a woman or child as only a fraction of those of a man. There is no distinction in Sir John Orr's work between adults and children, though in the foreword of his book he points out that the group with the lowest income and which averages an expenditure of four shillings per head per week on food "contains a disproportionately high number of children." It is the purpose of this paper to investigate in more detail this aspect of nutrition which puts a large proportion of children in the class which is grossly under-nourished, and especially to consider whether this effect is due to assuming that a child requires as much expending upon its food as an adult.

For this purpose 715 budgets which recorded expenditure on food by members of the Women's Co-operative Guild during one week in April, 1935, have been utilised. These budgets were specially collected for Sir John Orr's investigation and he used 538 of them in the 1,152 budgets from which he inferred the food consumption habits of the people.

These 715 budgets contain too many large families, too many children of school age, and too few infants under five to be really typical of the families of the country. Presumably women with adult children have more time, and women with babies less time, than normal wives to belong to Co-operative Guilds. Only 4½ per cent. of the persons included in the budgets were infants under five, whilst the corresponding percentage in the 1931 census was 7½. In 1931 15 per cent. of the population was between five and fourteen, 28 per cent. of the people in the budgets were between those ages. A comparison of the sizes of the families can best be shown by means of a table :—

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[1] Sir John Orr, *Food, Health and Income.*

Number of persons in family "x."	Percentage of families with "x" persons	
	1931 Census	Guild Budgets
1	7	–
2	22	1
3	24	6
4	19½	15
5	12½	21
6	7	22
7	4	16
8	2	11
9 and more	2	8
	100	100

Because of these differences no attempt is made here to generalise. It is a statement of conditions which existed in April, 1935, in 715 particular households as declared by members of those households.

The plan followed is to divide families according to the number of children they contain, defining a child as any person under 14 years of age. Further the children are sub-divided according to whether they are under five years old or not. This is convenient as it roughly divides the children according to whether they come under the care of the school medical officers with the possibility of free meals or milk, or whether they do not have such care. In the latter case they may, but probably do not, have care from child welfare centres.

Given this primary division the families are divided as Sir John Orr divides them, according to expenditure per person on food. Then two further analyses are made allowing for reduced costs of food for children.

In 1933 the British Medical Association were concerned with the problem of determining the "minimum weekly expenditure on foodstuffs which must be incurred by families of varying size if health and working capacity are to be maintained."[1] They discredited a "bare" diet on grounds of monotony and inadequacy of vitamins and minerals. They supported a diet costing 5s. 10½d. in June, 1933, for an adult male of over 14 and calculated cost coefficients for women and children :—

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[1] British Medical Association, *Report of Committee on Nutrition.*

	Cost per head per week Pence
Adult male over 14	71
Adult female over 14	59
Child 12 and under 14	64
„ 10 „ 12	57
„ 8 „ 10	50
„ 6 „ 8	43
„ 3 „ 6	41
„ 2 „ 3	37
„ 1 „ 2	32

This paper, on the basis of the above scale, and allowing for a small rise in the cost of food by April, 1935, assumes that the average food cost in that month was :—

For an adult	68d.
For a child (5–14)	55d.
For an infant (0–5)	39d.

If families spent less than this amount on food they are defined as living in poverty (definition A). A harsher definition is also utilised (definition B) which reduces still further the allowance for children :—

For an adult	68d.
For a child (5–14)	51d.
For an infant (0–5)	27d.

This harsher definition is worked out on a basis of calorie requirements only and makes no allowance for different types of food for young children.

The results show that in these households poverty is associated very strongly with the number of children, no matter which definition of poverty is used.

Number of Children in Family	Number of Families	Percentage of the Families in Poverty		
		Definition "A"	Definition "B"	Less than 5s. per Head expended on food
0	144	5	5	2
1	179	6	4	3
2	167	16	13	11
3	104	24	20	22
4	67	45	36	44
5	37	65	49	70
6	10	90	80	100
7 and more ..	7	100	86	100
All Families ..	715	19	16	17

This means that the proportion of children who are living in families with the lowest standard of nourishment is greater than the proportion of adults in such families. Under definition "A" 19 per cent. of the families are in poverty. Only 16 per cent. of the adults are in such families although those families include 35 per cent. of the children under fourteen, and 51 per cent. of the infants under five years old. This exceptionally high proportion for infants is partly due to the character of the sample, for most of the infants belong to families where there are several other children.

	Percentages			
	Adults	Children Aged 0–14	All Persons	Infants Aged 0–5
In Poverty—Definition "A" ..	16	35	22	51
Definition "B" ..	13	28	18	40
With following expenditures per head on food :—				
*4s.	13	34	20	55
6s.	30	35	31	30
8s.	32	22	29	12
10s.	18	7	15	2
12s.	7	2	5	1
	100	100	100	100

* 4s. includes 4s. 11d. or less. 6s. includes 5s. to 6s. 11d., etc.

In these households poverty shows itself to be a function primarily of parenthood, rather than of low wages. By becoming parents many of these people have sacrificed food, which is what one would expect in a country where the family income remains fixed when the number of children increases. In these budgets the average family expenditure upon food did not remain constant but declined when there was more than one child in the family. This decline was associated with a decline in the average number of adults as the number of children increased. When there are a number of young children there are usually fewer children who have attained the age of fourteen, and who can thus make a contribution to the family income.

Number of Children in Family	Number of Families	Average Expenditure on Food		Average Number of Adults per Family
		Per Family Pence	Per Person Pence	
0	144	511	106	4.8
1	179	526	97	4.4
2	167	502	85	3.9
3	104	457	71	3.4
4	67	488	66	3.4
5	37	436	54	3.1
6 and over ..	17	412	43	3.0
Total	715	497	83	4.05

This sample of budgets suggests that the problem of nutrition can best be solved by special treatment of all persons who are members of families with a number of children. To give special treatment to the children alone would be insufficient.

In the tables which follow fuller details are given of the 715 budgets.

DISTRIBUTION OF FAMILIES ACCORDING TO CONSTITUTION AND EXPENDITURE ON FOOD.

Constitution of Family		Number of Families	Families in Poverty		Expenditure per Head on Food.*					
Adults	Children		"A"	"B"	4s. (Under 5s.)	6s. (5s. and under 7s.)	8s. (7s. and under 9s.)	10s. (9s. and under 11s.)	12s. (11s. & over)	Av'ge Pence
2	–	6	1	1	1	1	1	1	2	108
3	–	22	2	2	1	2	5	9	5	112
4	–	31	–	–	–	3	12	6	10	113
5	–	37	1	1	–	4	15	12	6	107
6	–	32	2	2	1	5	13	11	2	100
7	–	12	1	1	–	6	2	2	2	93
8	–	3	–	–	–	–	1	1	1	117
11	–	1	–	–	–	–	–	1	–	120
Without Children		144	7	7	3	21	49	43	28	106
2	1	18	1	1	1	6	6	3	2	97
3	1	37	5	5	4	7	11	12	3	97
4	1	43	1	–	–	10	25	7	1	94
5	1	39	3	2	1	12	11	10	5	99
6	1	24	–	–	–	4	15	4	1	96
7	1	12	–	–	–	3	6	1	2	102
8	1	5	–	–	–	2	1	2	–	98
11	1	1	–	–	–	–	1	–	–	99
1 Child		179	10	8	6	44	76	39	14	97
2	2	37	5	2	3	13	16	4	1	85
3	2	39	4	3	3	13	16	5	2	89
4	2	33	8	7	5	11	11	6	–	82
5	2	30	5	5	3	11	11	5	–	84
6	2	19	3	3	3	10	3	3	–	81
7	2	4	2	1	1	2	–	1	–	77
8	2	3	–	–	–	1	1	1	–	92
9	2	2	–	–	–	1	–	–	1	105
2 Children ..		167	27	21	18	62	58	25	4	85
2	3	34	8	7	9	22	2	1	–	65
3	3	25	7	4	5	18	2	–	–	68
4	3	21	4	4	3	8	10	–	–	76
5	3	16	4	4	4	6	6	–	–	75
6	3	5	2	2	2	2	–	1	–	72
7	3	3	–	–	–	2	1	–	–	75
3 Children ..		104	25	21	23	58	21	2	–	71

* The columns give the numbers of families to which the rate of expenditure applies. The last column gives the average expenditure on food in pence per head.

DISTRIBUTION OF FAMILIES ACCORDING TO CONSTITUTION AND EXPENDITURE ON FOOD (*continued*).

Constitution of Family		Number of Families	Families in Poverty		Expenditure per Head on Food.*					
Adults	Children		"A"	"B"	4s. (Under 5s.)	6s. (5s. and under 7s.)	8s. (7s. and under 9s.)	10s. (9s. and under 11s.)	12s. (11s. & over)	Av'ge Pence
2	4	28	14	12	15	10	1	2	–	60
3	4	13	7	5	6	5	2	–	–	60
4	4	10	4	3	4	4	2	–	–	64
5	4	10	5	4	4	4	2	–	–	67
6	4	4	–	–	–	1	2	1	–	89
8	4	2	–	–	–	–	1	1	–	102
4 Children ..		67	30	24	29	24	10	4	–	66
2	5	14	9	7	11	2	1	–	–	51
3	5	13	10	7	10	3	–	–	–	50
4	5	6	4	4	4	2	–	–	–	50
5	5	1	–	–	–	–	1	–	–	88
6	5	2	1	–	1	1	–	–	–	68
7	5	1	–	–	–	1	–	–	–	72
5 Children ..		37	24	18	26	9	2	–	–	54
2	6	4	3	3	4	–	–	–	–	47
3	6	4	4	3	4	–	–	–	–	43
4	6	1	1	1	1	–	–	–	–	52
5	6	1	1	1	1	–	–	–	–	27
6 Children ..		10	9	8	10	–	–	–	–	43
2	7	3	3	2	3	–	–	–	–	45
4	7	1	1	1	1	–	–	–	–	44
5	7	1	1	1	1	–	–	–	–	40
3	8	1	1	1	1	–	–	–	–	36
4	8	1	1	1	1	–	–	–	–	46
		7	7	6	7	–	–	–	–	43

* The columns give the numbers of families to which the rate of expenditure applies. The last column gives the average expenditure on food in pence per head.

DISTRIBUTION OF FAMILIES ACCORDING TO EXPENDITURE PER HEAD ON FOOD

Number of Children in Family	Number of Families	Families in Poverty		Expenditure per Head on Food.				
		Definition "A"	Definition "B"	4s. (Under 5s.)	6s. (5s. and under 7s.)	8s. (7s. and under 9s.)	10s. (9s. and under 11s.)	12s. (11s. and over)
0	144	7	7	3	21	49	43	28
1	179	10	8	6	44	76	39	14
2	167	27	21	18	62	58	25	4
3	104	25	21	23	58	21	2	–
4	67	30	24	29	24	10	4	–
5	37	24	18	26	9	2	–	–
6	10	9	8	10	–	–	–	–
7 and more ..	7	7	6	7	–	–	–	–
All	715	139	113	121	218	216	113	46

DISTRIBUTION OF ADULTS (OVER 14) ACCORDING TO EXPENDITURE PER HEAD ON FOOD

Number of Children in Family	Number of Adults	Adults in Poverty		Expenditure per Head on Food.				
		Definition "A"	Definition "B"	4s.	6s.	8s.	10s.	12s.
0	698	32	32	11	112	240	212	123
1	793	36	27	19	194	351	167	62
2	657	111	91	75	255	205	105	17
3	358	85	74	77	186	87	8	–
4	225	90	71	84	77	46	18	–
5	115	70	51	74	34	7	–	–
6	29	27	24	29	–	–	–	–
7 and more ..	22	22	20	22	–	–	–	–
	2897	473	390	391	858	936	510	202

DISTRIBUTION OF CHILDREN (5–14) ACCORDING TO EXPENDITURE PER HEAD ON FOOD AND CONSTITUTION OF FAMILY.

Number of Children in Family	Number of Children Aged 5–14	Children (5–14) in Poverty		Expenditure per Head on Food.				
		Definition "A"	Definition "B"	4s. (Under 5s.)	6s. (5s. and under 7s.)	8s. (7s. and under 9s.)	10s. (9s. and under 11s.)	12s. (11s. and over)
1	173	10	8	6	42	73	39	13
2	309	47	37	30	115	108	48	8
3	272	65	55	57	151	58	6	–
4	228	100	83	95	82	36	15	–
5	136	86	63	93	36	7	–	–
6	45	41	36	45	–	–	–	–
7 and over ..	39	39	34	39	–	–	–	–
	1202	388	316	365	426	282	108	21

DISTRIBUTION OF CHILDREN (0–5) ACCORDING TO EXPENDITURE PER HEAD ON FOOD AND CONSTITUTION OF FAMILY.

Number of Children in Family	Number of Children Aged 0–5	Children (0–5) in Poverty		Expenditure per Head on Food				
		Definition "A"	Definition "B"	4s.	6s.	8s.	10s.	12s.
1	6	0	0	0	2	3	–	1
2	25	7	5	6	9	8	2	–
3	40	10	8	12	23	5	–	–
4	40	20	13	21	14	4	1	–
5	49	34	27	37	9	3	–	–
6	15	13	12	15	–	–	–	–
7 and over ..	12	12	10	12	–	–	–	–
	187	96	75	103	57	23	3	1

"Essays in the Theory of Employment"[1]

This book is composed of a collection of essays which Mrs. Robinson describes as representing an attempt to apply the principles of Mr. Keynes's *General Theory of Employment, Interest and Money* to a number of particular problems.

The collection opens with an essay on *Full Employment.* On the definition of full employment Mrs. Robinson does not quite see eye to eye with Mr. Keynes. She finds difficulties in the way of the conception of a critical psychological point at which the real wage ceases to cover the disutility of labour, and prefers to define the point of full employment as that " at which every impediment on the side of labour to a rise in money wages finally gives way ", the " impediments " being the competition of those seeking employment.

This essay on full employment starts with a study of the effect of an unduly low rate of interest at a time of full employment in carrying an inflationary rise of prices and wages beyond any definable limit. Mr. Keynes said that the rate of interest could not be in equilibrium at a level below the rate which corresponds to full employment, because " a state of true inflation " would be produced. Mrs. Robinson follows up the line of investigation there indicated, assuming that " the rate of interest is reduced and is maintained continuously at the lower level " (p. 17). She adopts unquestioningly Mr. Keynes's assumption that the volume of investment will be adjusted exactly and promptly to the rate of interest, and infers that the reduction in the rate of interest raises the price of capital goods, that a rise of wages in the capital goods industries must follow, that this will lead to increased consumption and a rise of wages in the consumption goods industries, and that so the vicious circle will be joined.

That is on the assumption that the reduction in the rate of interest is maintained in the face of a considerable rise of money wages, a case which, she points out, cannot normally occur. " A rise in prices and incomes leads to an increase in requirements for money balances in the active circulation. This tends to reduce the amount available for inactive balances, and so causes the rate of interest to rise. . . . The rise in interest checks investment and continues to the point at which sufficient investment occurs to prevent a further rise in wages " (p. 25). Such is the " lively sense of the horrors of inflation sucked up with the milk of the mothers of bank presidents ", that " the process of checking a rise in money wages is usually set to work long before the point of full employment is actually reached ".

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[1] *Essays in the Theory of Employment.* By Joan Robinson. Macmillan, 1937. vii + 255 pp. 8s. 6d.

Mrs. Robinson proceeds to discuss " the ideal policy for monetary authorities to pursue " (p. 29). She is led to pessimistic conclusions by her assumption in regard to the policy of Trade Unions. " It is idle," she says, " to attempt to reduce such questions as Trade Union policy to a cut-and-dried scheme of formal analysis, but it is plausible to say, in a general way, that in any given conditions there is a certain more or less definite level of employment at which money wages will rise, and a lower level of employment at which money wages will fall " (p. 7). From this very tentative generalisation Mrs. Robinson is led to the inference that a monetary policy intended " to maintain a high level of employment " will involve a cumulative increase in the quantity of money, so that " prices will move constantly upwards ". For, even though the monetary authorities are presumed " to leave some margin of safety and to aim at a level of employment somewhat short of full ", yet, " if Trade Unions are powerful, this will entail that employment lies above the upper critical level, at which money wages rise " (pp. 30–1).

A policy of stable prices " is by no means equivalent to maintaining stable employment ", because, if efficiency changes, there must be a compensatory change in wages. " If efficiency is increasing . . . stability of prices then requires that the level of employment shall be held sufficiently high to induce just that rate of rise in money wages which will offset the effects of increasing efficiency, and the level of employment must be made to fluctuate with the rate of industrial progress " (p. 34). The policy of maintaining a constant level of money incomes is even more deleterious, " for it would require that employment should be kept below the upper critical level even when there is an increase in efficiency " (p. 35).

The conclusion that the monetary authorities are powerless to avoid unemployment is reached simply and solely because the " upper critical level " of employment at which the Trade Unions insist on a rise of wages is something short of " full employment ". The whole black fabric of Mrs. Robinson's pessimism is built up on what " it is plausible to say, in a general way " is the practice of Trade Unions. Not a shred of evidence is produced to show that it actually is their practice. If past experience is a guide, the most consistent characteristic of Trade Union practice, so far as the general level of wages is concerned, is the *lag* of both increases and decreases of wages behind the fluctuations of activity and of price levels which justify them. Rises and falls of wages alike, far from being premature, have usually been overdue.

This may not be so in the future. Perhaps there is a danger that a short-sighted insistence on rises of wages, regardless of the consequent rises of prices, may become the fashion. But the mere possibility does not justify an economist in erecting such insistence into an economic principle. Economists complain that people who are engaged in practical affairs, among whom Trade Union leaders may be reckoned, do not take their advice. But what is the use of advice which takes the irrational behaviour of the Trade Union leader himself as axiomatic? Mrs. Robinson, accepting the conclusion that it is by monetary policy that the volume of employment is primarily determined, asks us to reject every proposal for so directing monetary policy as to secure full employment on the ground of her unsupported view of Trade Union policy.

Nor is her view of the congenital tendencies of bank presidents supported by experience. In every one of the financial crises from 1825 to 1907 it is, I think, quite clear that banking systems intervened to restrict credit too late. That is equally true of the inflation of 1919–20. And the flounderings and cross purposes of the years 1924–30 do not afford much evidence on the subject.

The second essay in the book deals with the Mobility of Labour. It is a well reasoned and in many respects useful analysis. But it introduces us to an application of Mr. Keynes's theory in an uncompromising form, which goes beyond what Mr. Keynes himself holds. According to him it *may* happen that the rate of interest low enough for idle balances to be zero is too high for full employment. That would be so if the amount of investment corresponding to a marginal efficiency equal to the rate of interest were insufficient to employ the savings determined by the propensity to consume in conditions of full employment. Far from saying that this always occurs, Mr. Keynes thinks that in the 150 years ending with 1914 a relatively high marginal efficiency of capital "allowed a reasonably satisfactory average level of employment", and that if it is operative "to-day and presumably for the future" that is only because the "schedule of the marginal efficiency of capital is, for a variety of reasons, much lower than it was in the nineteenth century" (*General Theory*, pp. 307–8).

The latter statement expresses Mr. Keynes's opinion, but it is not free from doubt. Since the marginal efficiency of capital is equal to the rate of interest, and the rate of interest is higher than it was forty or fifty years ago, there is some reason to suppose that we are still enjoying the same immunity from the effects of an excessive liquidity preference as at any time from the days of Adam Smith to 1914.

Yet Mrs. Robinson assumes quite unconditionally that an increase of thriftiness (the propensity to save, complementary to the propensity to consume) will be associated with a decrease of demand and so of employment.

Any increase in profits may be assumed to increase thriftiness, and she decides to "treat a reduction in the amount of dole payments corresponding to a given level of output as constituting an increase of thriftiness ". Her subtle and ingenious mind revels in the resulting crop of paradoxes.

Her chapter on proposed remedies for unemployment (shorter hours, higher school leaving age, emigration, subsidies in aid of wages) affords full scope for this propensity. And it is further removed from any contact with the facts of practical life by the assumption that " any monetary repercussions of the schemes are offset by appropriate action by the banking system and that the rate of interest is unaffected by them " (p. 62). If hours are reduced and hourly wages are raised in proportion, then " since the change of wages is expected to be permanent, the marginal efficiency of capital is unaffected. It follows that . . . a change in money wages, in itself, produces no effect upon employment or upon real wages " (pp. 63–4). The banking system is assumed to intervene so to raise the price level as to prevent the marginal efficiency of capital (which depends on profits) being diminished, but not to be willing to *increase* marginal efficiency in this way if there were no rise of money wages, for " if the object in view were merely to produce an effect on employment by changing the rate of interest, simpler methods could be used ". But these " simpler methods " have already been ruled out by the monetary authorities' presumed abhorrence of inflation, and the Trade Unions' insistence on premature rises of wages. Why is the possibility that the rise in hourly wages may be accompanied by no corresponding rise in the supply of money or in the price level not considered ?

Within the limits of these assumptions, what is left ? If investment be assumed constant, " the reduction in hours must lead to a decline in output ", because the consequent improvement in employment and reduction in the dole will increase thriftiness. So " if output were constant total saving would be increased. Therefore if output were constant, consumption would decline. It follows that output cannot be constant but must decline ".

There may be changes in the rate of investment, but they will persist only for a certain time, while the effect of increased thriftiness will be permanent.

A chapter on the Long-period Theory of Employment is devoted to " the background of equilibrium theory " which corresponds to the short-period analysis developed by Mr. Keynes, and which " is largely unexplored ". This equilibrium relates to a closed community " with population stable in respect to numbers and to age distribution and with given tastes and technical knowledge " (p. 105). This is not " a situation which would ever be likely to occur in actuality ", but " the situation of any actual community can be regarded as a situation in which investment is tending towards zero " (p. 136).

If, in the conditions assumed, " a certain rate of interest has been established and is maintained at an unvarying level ", investment must eventually come to an end, " the stock of capital is adjusted to the given rate of interest and no further accumulation takes place". Investment being zero, saving must be zero, and output and national income must be such as to bring this about. "The amount of unemployment, in equilibrium, must be whatever is sufficient to reduce net saving to zero" (p. 110).

The next step is to examine the effect on the equilibrium position of a reduction in the rate of interest, taking account of the effects not only on the period of production and on output, but also on thriftiness. Mrs. Robinson evolves a formula to show whether there results an increase or a decrease in employment. The formula depends on the "elasticity of substitution between labour and capital", and it follows (apparently) that a fall in the rate of interest may actually reduce the equilibrium level of employment (p. 119). Consequently it is "impossible to argue that there is any self-righting mechanism in the economic system which makes unemployment impossible, even in the longest of runs" (p. 121).

This pessimistic conclusion is reached on the assumption that long-period equilibrium must take the form of zero investment and zero saving, that is to say, a glut of capital. A reduction of the rate of interest *would for a time* revive investment and permit saving; it is when investment has once again reached its limit that there is no presumption that a lower rate of interest will mean greater activity.

So long as the rate of interest can go on being reduced, the glut of capital can be postponed. And it will be still further postponed so long as new discoveries and inventions are providing more remunerative openings for investment. Inventions *may* be capital-saving rather than labour-saving (p. 132). But even capital-saving inventions are likely for a time to give rise to increased net investment, and "a sufficiently rapid succession of inventions, provided they are not extremely capital-saving, would prevent the rate of investment from ever falling to zero" (p. 136).

"The situation of any actual community can be regarded as a situation in which investment is tending towards zero." Perhaps, but alternatively, if invention (especially labour-saving invention) is sufficiently active, it might be regarded as one in which investment is tending away from zero. And even when investment is tending towards zero, the problems envisaged in this essay do not become relevant so long as investment can be maintained by a falling rate of interest.

The contingency of an actual glut of capital, even if it be remote, is one which economists ought to take into account. But it is hardly worth while to elaborate in detail the circumstances of a community in which a sufficient degree of impoverishment to reduce saving to zero has been passively accepted as the appropriate consequence. It ought not to be very difficult to devise institutions which would either permit individual saving without the corresponding accumulation of wealth, or reduce individual saving within narrow limits.

Mrs. Robinson, referring to a passage in Mr. Keynes's *General Theory* (p. 48), says that he uses " long-period equilibrium " in a slightly more extended sense than hers (p. 107). Rather he uses it in a totally different sense, for in that passage he is dealing with the adjustment of employment to long-period expectations, whatever the state of investment might be. The case of zero investment he refers to elsewhere (*General Theory*, pp. 217–20, 323–4). He apparently looks forward to a state of things in which the marginal efficiency of capital will have fallen practically to zero, but investment will nevertheless continue without resort to " digging holes in the ground ". If public and private investment are alike limited (*General Theory*, p. 106) this is no solution of Mrs. Robinson's problem.

It is a matter for regret that Mrs. Robinson is tempted to apply her great powers of economic analysis and reasoning to matters so remote from real life.

R. G. HAWTREY.

Book Reviews

Population Pressure and Economic Life in Japan. By Ryoichi Ishii. P. S. King & Son. 1937. xix + 259 pp. 12s. 6d.

Japanese population figures are a source of embarrassment to those who, possessing no first-hand knowledge of Japanese sources, have to rely upon statistics taken from year-books and other similar publications. It is hard to discover the manner in which the figures are compiled and so to form any opinion as to their validity. Puzzles are presented by the fact that series of figures given in official publications, purporting to show the population of the country at various dates, do not agree. To students who are in this position this book will be most welcome. The author, whose work was done in the United States, has given a full and lucid account of the methods employed to obtain figures at different times both of the population and of births, marriages and deaths. Thus we find that the discrepancies mentioned above between the population figures are due to the existence of three series of figures up to 1920, the first being derived from an enumeration by domiciles, the second being due to an attempt to revise the first series, and the third being due to another similar attempt. The last series seems to be the best founded and differs widely from the first; for instance it gives a figure for 1872 which is 1,700,000 less than that in the first, and a figure for 1920 which is 2,400,000 greater than that in the first series for the same year. These wide differences explain some curious variations between the figures given in different publications for vital statistics, since birth- and death-rates work out differently according as to which series of population figures is employed. Again we learn that married women, as recorded before 1920, did not include those "informally married", i.e. those whose marriages were celebrated by social and religious ceremonies but not registered. Since 1920 both classes of women have been recorded as married. This lays a trap into which certain students, for instance Mr. Crocker in his excellent study of the Japanese population problem, have unwittingly fallen. Later chapters give useful details of the distribution of population, of occupational analysis, of food supply and of industrial production. There are also chapters on migration and on birth-control. The latter is especially informative; Mr. Penrose and others have described the spread of propaganda in favour of contraception, but it is interesting to have information about the recent spread of abortion and of the change of attitude towards it. We are told that "there is no doubt that the general public condonation of the practice of abortion will vitally affect Japan's immediate population growth—despite the existing legal prohibitions".

As regards the Tokugawa era the author holds the view that "abortion and infanticide were the most effective agents" in keeping the population stable. In the Meiji period "the prohibition of abortion and infanticide was instituted as a step in a national programme of modernisation rather than as a policy intended to increase the population". Nevertheless it is to the disappearance of these practices that the huge growth in recent decades of the population of Japan is due. It is well known that from 1872 until about fifteen years ago the birth-rate was rising, since when it has fallen. This increase was not due to better registration, at least not in the sense in which better registration caused an apparent rise in the birth-rate of some European countries. What seems to have happened is that infanticide took a long time to disappear and that the births of children, who were not allowed to live, were not recorded. The fall in the birth-rate in recent years is due to birth-control, i.e. to the introduction of contraception and to the reappearance of abortion.

When dealing with changes in outlook, customs and practices, the author is very interesting and instructive. But, while he is also a guide who inspires confidence when presenting figures, he is less well equipped to analyse them, and his attempts in this direction are somewhat rudimentary. In fact, he is a historian and a sociologist rather than a statistician and an economist. These limitations, however, scarcely detract from the value of the book, which is mostly devoted to aspects of the matter with which he is fully competent to deal. The book is a most useful addition to population literature.

A. M. CARR-SAUNDERS.

Economic Essays. By CHARLES JESSE BULLOCK, Cambridge, Mass.: Harvard University Press. 1936. viii + 550 pp. $5.00.[1]

Professor Bullock has recently retired from active teaching in Harvard University and from his pre-eminent position in the Harvard Economic Service, after thirty-one years in the Department of Economics. It is hoped that he will be able to devote his unaccustomed leisure to the subjects which have always interested him, but which the arduous tasks he has undertaken have forced him to put aside. In London we gratefully remember that it was largely due to his interest and co-operation that our Economic Service was successfully launched.

Meanwhile his friends and former students have collected and published twenty-three papers by him, selected from some ninety articles of which a bibliography is given. Professor Bullock has written only two books on the Elements of Economics; his main

[1] Also Oxford University Press. Price 21s.

contributions are only to be found scattered through various Journals and Reports. They cover a wide field, but relate principally to problems of taxation, with special relation to American conditions. These are of only moderate interest to English readers, and since they range from 1898 onwards, many of them are out of date, and what is of lasting importance must have already had its influence on subsequent thought and writings. But in the first group of Essays, as here arranged, there is a considerable amount of valuable critical study of the statistics of trade, prices and wages over a long period, which ought not to be overlooked by anyone working in that field. In last century, at least, many of the American official statistics were imperfect in collection and issued in such forms as to give rise to erroneous conclusions. These Essays are very helpful in separating the wheat from the chaff.

Professor Bullock is, or has been, a classical scholar, and the last three Essays deal with the economics of Athens and of Dionysius, with an interlude on an ancient Banking advertisement.

"To citizens and foreigners this bank gives equal dealing;
Deposit and withdraw, when your account is correctly made up.
Let another make excuse: but Caicus even at night
Pays foreign money to those who want it."

Such is Professor Bullock's translation of an Epigram by Theocritus. The most entertaining of these Essays is on another ancient story, that of Solomon in all his extravagance and Rehoboam with his resulting difficulties. In this and the Greek studies we are invited to draw analogies with modern developments in the United States. ". . . and there is no new thing under the sun. Is there a thing whereof it may be said, See, this is new? it hath been long ago, in the ages which were before us."

A. L. Bowley.

The Westminster Bank Through a Century. By T. E. Gregory. Oxford University Press. London: Humphrey Milford. 1936. Two volumes. xii + 396 + vi + 355 pp. 30s.

Students of banking already owed a debt to Professor Gregory; and in these beautifully printed and illustrated volumes he wins our gratitude once more. It must have been an attractive task for the editor and commentator of the *Select Statutes, Documents and Reports Relating to British Banking 1832–1928* to write the biography of one of the leading British joint-stock banks. He has carried out this task in an admirable manner. The volumes before us are a monument of industry and scholarship. They contain a wealth of information, for the professional banker as well as for the student, and deserve to be widely read and studied.

The construction of a book like this one presents no small difficulty. Recounting the history of a large banking concern which has gradually developed out of a considerable number of constituent parts necessarily involves the immersion in a multitude of details. At the same time the environment of external circumstances of the epoch in which the subject of the story lived and grew has to be depicted in broad and forceful lines, so that the course of development may be shown in its true perspective. These two elements of the narrative have to be welded together into one organic whole, and it requires a refined sense of proportion not to place too much emphasis either on the one or on the other.

Professor Gregory has solved this difficulty supremely well. In his opening chapters he sketches the background of English banking in the period prior to the Bank Charter Act of 1833, and gives an interesting account of the negotiations with the Bank of England and the discussions in Parliament which preceded the passing of that Act with its celebrated "declaratory clause". This clause, as is well known, definitely established the right of any body, society, company or partnership, although consisting of more than six persons, to carry on banking business in London or within sixty-five miles thereof, provided that it did not issue notes. The third reading in the House of Commons of the Bill which was to become the Bank Charter Act of 1833 took place on August 19th of that year, but a few days before that date the first active steps were already taken for the establishment of a joint-stock bank in London. Professor Gregory describes in detail how, as a result of these steps, the London and Westminster Bank was born. He then depicts in vivid colours the battles the new bank had to fight during the first two decades of its existence. The Bank of England and the private bankers in London were equally opposed to it. For some time the new institution was not allowed to accept bills; a drawing account at the Bank of England was refused to it; and it was excluded from the Clearing House. But as time went on all this changed and by 1854, the year of the admission of joint-stock banks to the Clearing House, the "struggle for recognition" had been won, and the London and Westminster Bank was firmly established.

At this point Professor Gregory very appropriately interrupts his story in order to deal, as he modestly phrases it, with some general considerations bearing upon the course of development of the modern English banking structure. The chapter which contains these general considerations is headed "Factors of Change". It is undoubtedly the best one in the book. The main features of the development of English banking generally from about 1850 onwards are set forth here in less than forty pages with a mastership that can only be acquired by profound knowledge and understanding. After this the tale goes on. The progress of the London and Westminster Bank, its country connections, the principles of its business, its absorptions, amalgamations, branch policy and balance sheet position are described. The author then turns to the history of the two other main constituents of the Westminster Bank of to-day: the London and County Bank, which was amalgamated with the London and Westminster Bank into the London County and Westminster Bank in 1909, and Parr's Bank, the amalgamation with which took place in 1918. The story of these two banks with their respective absorptions is also told in detail. An "Epilogue" deals with the completion of the amalgamation process, branch expansion and the balance sheet position of the Westminster Bank as we know it. It is all excellently done, and Professor Gregory is able on several occasions to bring to light new facts and details hitherto unknown.

The work concludes with six biographical sketches of prominent personalities in the Westminster Bank and its constituent institutions: Vincent Stuckey, Lord Overstone, Sir David Salomons, James William Gilbart, Walter Bagehot and Walter Leaf. From an academic point of view the essays on Lord Overstone and Walter Bagehot are by far the most interesting ones, but they all make pleasant reading and lend much colour to the picture presented in this valuable book.

A. M. de Jong.

Predecessors of Adam Smith. The Growth of British Economic Thought. By E. A. J. Johnson. New York: Prentice-Hall, Inc. 1937. xii + 426 pp. $3.50.

It is a somewhat mixed company which we find assembled in the present volume as predecessors of Adam Smith. Most of them are predecessors only in the sense that they discussed economic problems before Adam Smith, and they have been selected because the author regards them as particularly representative of the different strands of thought which dominated sixteenth and seventeenth century discussions of economic problems. As the author himself has evidently not much love for what he calls the "skeletal logic" which forms the backbone of modern economics, he is less interested to trace the developments of the ideas which were ultimately embodied into a systematic science of economics. His interest belongs rather to the views which mercantilist writers had on questions of economic policy, particularly to their various suggestions to increase productivity. Side by side with discussions of the great figures of Mun, Petty, or Hume, or of well-known names as Hales, Misselden, and Steuart, we find chapters devoted to Nehemia Grew, an early prototype of modern "scientific" planners and efficiency fanatics, but altogether uninteresting as an economist, or to Charles King, who, although undeniably influential as editor of the *British Merchant*, was hardly anything more than a successful propagandist. If the significance of their contributions to economic thought had been made the criterion of selection, surely a different list of names should have resulted. But as an illustration of the sort of argument which had the ear of the public of their time, the collection does not serve badly.

The discussion of the work of the different writers is careful and scholarly and brings much useful information together. Particularly for the chapter on the industrious M. Postlethwayt and the work of wading through all his voluminous publications all historians of economic thought should be grateful. It is less easy always to agree with the author's judgment and more than one reader will probably feel that the biographer's sympathy for the mercantilists has led him to be somewhat unjust to David Hume, "the Synthetist", as our author calls him.

The exposition suffers from a considerable amount of repetition, evidently due to the fact that more than a third of the book had originally been written for publication as separate articles and has not been sufficiently revised for inclusion in the volume. A word may be added here on the fashionable but annoying practice of relegating the "footnotes" to an appendix. This may be unobjectionable if the notes only give the exact reference for material used in the text. But to find that nine times out of ten recourse to the end of the volume produces only the number of a page, while the tenth time it yields a quotation illustrative of the main argument or even essential for its full appreciation, certainly does not make reading more pleasant.

F. A. HAYEK.

Explorations in Economics. Notes and Essays contributed in Honour of F. W. TAUSSIG. McGraw-Hill Publishing Company, Ltd. 1936. xii + 539 pp. 30s.

Among the considerable number of *Festschriften* which have appeared during the past few years the present one occupies a very high place, well worthy of the great figure whom it is intended to honour. Editors and contributors have combined to produce a collection of "notes and essays" of unusual interest; and this must be regarded as a particularly great tribute to the inspiration derived from Professor Taussig's teaching, since the contributors have not been drawn from the whole body of his numerous admirers but co-operation has been confined to those who at one time or another, as students or colleagues, have been in close personal contact with him. What deserves particular praise is that the editors have not only wished to keep the individual contributions short but have actually succeeded; in 534 pages the contributions of forty-eight authors have been collected and only one of them exceeds twenty pages; and the conviction of the editors "that something important can be said in ten pages" has in almost all instances been confirmed.

The volume is divided into three parts of approximately equal length, under the headings of "International Trade and Tariff Problems," "Wages and Capital," and "The Social Setting of

Economic Activity," which are representative of the main fields of Professor Taussig's interest. The first essays in each part, by Professors Viner, Schumpeter and Talcott Parsons respectively, give admirable surveys of Professor Taussig's own contributions to these subjects. It would be unfair to select for comment from the forty-five other contributions the few which might be discussed within the compass of a short review. Be it enough to say that the circle of Professor Taussig's friends and pupils seems to comprise a very substantial part of American economists from whom one expects interesting contributions to economic theory and that the standard reached is throughout what one would anticipate in such company.

The first section, as one would expect, is even richer in content and number of contributions than the two others. The title of the second is perhaps somewhat too narrow, since it covers the whole of the theory of distribution, including the theory of monopoly. The third covers a great variety of topics, from methodology to Laissez Faire and Socialism, the regulation of railway rates, Public Finance and Economic History.

At the end of the volume there is a complete bibliography of the writings of Professor Taussig which will be particularly welcome to many readers and which testifies to an even wider range of interests on his part than that of which most of his younger colleagues will be aware.

F. A. Hayek.

India's Balance of Indebtedness, 1898–1913. By Y. S. Pandit. Allen and Unwin. 1937. 210 pp. 10s. 6d.

Mr. Pandit has undertaken an inductive study of India's balance of indebtedness along the lines of Professor Viner's well-known work on Canada. He has made a thorough study of the methods adopted by various authorities for estimating the amount of the various items composing such a balance, and of the relevant Indian data, whether actual records, or estimates made from time to time of items not included in actual records. This prevents him from following Professor Viner's example slavishly, and he shows considerable discrimination in his selection of the most appropriate methods and materials. He has thus succeeded in compiling an interesting, although admittedly far from accurate, estimate of the amount and composition of India's balance of indebtedness during the period 1898–1913. In addition he analyses clearly and convincingly the mechanism of adjustment of Indian trade balances under the gold-exchange standard.

The statistical analysis is, on the whole, careful and sensible, although the incomplete and unreliable nature of much of the relevant datum necessarily prevents some of the results from being much more than intelligent guesses. The crux of the analysis and the most difficult problem involved is the estimation of the amount of foreign capital imported. Mr. Pandit attempts to check his results by compiling and comparing indirect and direct estimates of borrowings from abroad, but no entirely satisfactory explanation is offered of the somewhat serious discrepancies revealed. One cannot help wishing that Mr. Pandit had suggested how more accurate and complete records could be compiled in the future.

The author's theoretical analysis of the interrelation of foreign loans and investments, the barter terms of trade, Indian and foreign price levels and price changes, and of changes in the volume of imports and exports, which on the whole supports the classical analysis, throws considerable light upon the currency controversies of the period, and upon the working of the gold-exchange standard. In particular it disposes conclusively of the oft-repeated contention that the relative rise in Indian prices during the pre-war period was due to the inflationary policy of the Government, arising out of the adoption of the gold-exchange standard, and that this proves the inferiority of the gold-exchange standard as compared with the full gold standard. Mr. Pandit demonstrates clearly that the method of adjustment under the gold-exchange standard does not differ from that under the full gold standard, except that in the former case a movement of Council Bills takes the place of a movement of gold—a difference in no way associated with inferiority, but which actually effects an economy, as the cost of moving Council Bills is less than that of transmitting gold.

The main conclusions are that during this period there were two main factors tending to disturb India's balance of indebtedness: i.e. an increased import of foreign capital and an enhanced world demand for India's principal exports. As a result of the increased purchasing power of India, due to these two factors, there was a rise in the price-level which, as world prices in general were also rising at the time, took the form not only of an absolute, but of a relative rise in the Indian level of prices. This rise was particularly great in the case of India's principal exports.

Adjustment to the two disturbing factors, therefore, occurred partly by means of a relative rise in the Indian price level, partly by means of an increased volume of Indian exports, and hence partly by means of an improvement in India's barter terms of trade, whilst the use of Council Bills obviated any movement of gold for the purpose of adjustment. India, of course, had a large net import of gold almost every year, and over the period as a whole, but this must be attributed to other factors than the need to adjust the balance. As, however, the two disturbing factors acted simultaneously it is impossible to determine how far the relative rise in Indian prices, and the other changes noted above, were due to the imports of foreign capital, how far to increased demand for India's exports.

VERA ANSTEY.

Economic Thought and Language. By L. M. Fraser. London: A. & C. Black. 1937. xx + 411 pp. 12s. 6d.

This is a critique of some fundamental economic concepts. It examines one by one such concepts as value, money, and capital, distinguishing the different meanings which each may bear and noting ambiguities. Professor Fraser claims that it is merely a study in logic. "If at times suggestions have found their way into my book as to improvements which might be effected in the substance or form of economic theory as presented in the current textbooks, these are incidental and subsidiary." Do not believe it. A book written on those lines would be pedantic and dull, and this book is interesting and alive. Professor Fraser has very clear beliefs as to what economics should do, and how it should do it. It is these beliefs which give a common direction and purpose to his discussions, making the book a unity instead of a series of disconnected essays.

It may be thought that his subject should be studied historically. Undoubtedly such an approach would be very enlightening. But the task which Professor Fraser has set himself is a different one. He surveys the different meanings which a term may bear, whether or not they have played a part in economic thought. The large number of meanings which he brings to light is a revelation. I am fairly sure that I shall not use value in the sense of negative relative esteem value or toy with the concept of short-lived communal recurrent-use goods, but now that he has shown me the wilderness Life will never again be quite the same.

The Promised Land towards which he guides us, exposing the many pitfalls which await us if we take the wrong turnings, is seen through a slight haze, but it clearly consists largely of the modern theory of value, in which money and the pricing of factors find their proper setting. How admirably he criticises the traditional classification into Land, Labour and Capital, the attempt to introduce Enterprise as a fourth category, and "the idea that rent and interest can be taken as mutually exclusive forms of income, the one derived from 'land', the other from 'capital'"! "A true factor class, like a true commodity class, must be composed of members which are for practical purposes interchangeable or 'substitutable'—which are, in fact, identical in all relevant respects." Golden words!

It is both distasteful and difficult to find something to disagree with in this admirable work, but that seems a convention of reviewing. For that reason alone I venture to protest against the proposal to discard the concept of "productivity" as "a survival from the older approach to the distribution problem". It is true that the values of factors of production tend to equal their marginal productivity only under perfect competition. But entrepreneurs buy factors because these add to their output and thereby to their receipts. Surely an analysis of factor prices should be preceded by a study of "the production function", showing how changes in the proportions of the factors affect the quantity of output ? It is then easy to show how much an additional unit of a factor adds to output and thereby, whether under perfect or imperfect competition, to receipts, and to apply the principle that the entrepreneur will try to equate marginal costs with marginal revenue.

Perhaps Professor Fraser's dislike of " productivity " is connected with his view that there is no " law " of diminishing returns. But there is. Given the state of technical knowledge, and the absence of relevant indivisibilities, then after a point an increased proportion of one factor in a combination of factors will reduce the marginal product of that factor. This is as certain as the law of gravity, which does not cease to be a law because feathers fly upward.

But these are minor points. I would urge all serious students of economics to read this work and especially, perhaps, the excellent chapter on capital.

F. BENHAM.

Einführung in die Grundlagen der Nationalökonomie. By RICHARD VON STRIGL. Vienna: Julius Springer. 1937. viii + 223 pp.

Die Lehre von der Wirtschaft. By WILHELM RÖPKE. Vienna: Julius Springer. 1937. vi + 195 pp. Rm. 5.70.

The Groundwork of Economic Theory. By J. WATSON. London: P. S. King & Son. 1937. x + 196 pp. 9s.

Elements of Economic Theory. By ERICH ROLL. London: Oxford University Press. 1937. viii + 276 pp. 5s.

Each of these four books is of high quality. I have no substantial criticism to offer of anything contained in them. My criticism is that each falls short of perfection, as a complete introduction to beginners, because it does not contain enough. Each writer seems to have set himself a kind of economic problem of his own: that of covering the field as well as he can within so many thousand words. Each has solved it, in his own way, very well. But the problem should never have been set. A review must be brief because the editor says so, but an author can make his book as long as he pleases: for him words are not scarce. In my view, the notion that an elementary book must be brief is a delusion. An advanced work, building on established conclusions and using concepts familiar to its readers, can afford the luxury of compression, but an introduction is written for readers quite new to the subject, so that everything must be carefully explained. Each of these books suffers, in one way or another, from being too short.

What should an elementary book on economics contain ? First, it should explain the nature of the economic problem. It should show that all wants cannot be fully satisfied and that a community must somehow decide how to use its limited resources, many of which are capable of alternative uses. It should discuss the production function and should show how wants can be more fully satisfied by taking advantage of division of labour, improvements in technical knowledge, and more capitalistic methods of production. All this is true however a community may be organised. Second, it should show how the economic problem is solved under capitalism. It should show how prices of all kinds are formed and how they are related to one another. It should explain the part played by the price-mechanism in directing men and other resources into those channels where their contribution to output is valued most highly. It should give some account of how the system responds to changes in tastes or in technique or in supplies of factors. If the resulting picture is to be at all life-like, some account of the part played by the State and other institutions, together with a certain amount of descriptive material and a number of illustrations from actual events, must be included. Third, I think a writer should remember that many people agree with Marshall that the question of poverty lends to economics its highest interest. He should say something about the influences affecting the wealth of nations. He should discuss the main factors, such as inheritance, giving rise to inequality of income. Finally, he might well say something about the possibilities and limitations of social reform. At any rate, he might consider the economic effects of certain leading types of State action, and how economic problems might be solved under some system other than capitalism.

All this forms a large programme and unless a book is fairly long something must inevitably be sacrificed. Some topics must be left out or dismissed far too briefly, or description and illustration must be cut down. Yet description is sometimes essential to give body to the bare bones of analysis. For instance, it seems to me a distinct weakness that none of these books discusses the supply of money within the framework of some existing monetary and banking system, or the demand for it in relation to the other types of assets available.

Professor Roll, possibly reflecting that he has already written a book about money, says practically nothing about money or banking or international trade, and Mr. Watson, who for all I know intends to write one, does the same. This at least saves space.

Professor Roll begins with an excellent account of the economic principle and then discusses its working and problems of change. His book is very readable. If he had maintained this form over a course about three times as long he would have written a fine text-book. He has failed only because the task is impossible within 80,000 words.

Mr. Watson perhaps escapes our strictures. For although he claims that his book is intended for students in the early stages of economic studies and that no previous knowledge of economic theory is assumed, he also says: "It has throughout been my aim to provide, not indeed a substitute for the standard works on economics, but rather a short introduction and guide to their understanding and appreciation." He considers a closed community, free from monetary disturbances, and confines himself to a thorough and systematic account of equilibrium analysis, without any padding or trimmings. His book is easily the stiffest of the four, and his style the most grumous. " In order to restrict the treatment to a length permitting a readier comprehension of the whole I have frequently resorted to brief tabulation," he explains. This work can doubtless be recommended to " serious students who do not shrink from the effort of sustained attention and systematic thought ", but I would advise them, *pace* Mr. Watson, not to take it at too early a stage.

As far as I can judge, the style of the two books written in German is excellent. They both attempt to cover the whole field. Of the two, Professor Röpke's very interesting study gives more space to the inequality of incomes and to social reform, and includes a brief discussion on communism. It may be thought that an elementary book should say nothing about policy, restricting itself to an account of how the economic system actually works. I agree that it is an abuse of trust for a writer to advocate policies, such as large expenditure on public works during any slump in any country, about which economists are not agreed and on which experience is far from conclusive, in an introductory book. But surely he should point out, for example, that if the distribution of property were different the whole system of prices and incomes would be different. There is a danger that a bald account of how prices are formed in an imaginary capitalist community will leave the impression that all attempts at intervention of any kind are misguided. In my view, all these books might have devoted more space to a discussion of the foundations and effects of monopoly power, as distinct from the theory of monopoly price.

Professor Strigl pays less attention than Professor Röpke to questions of policy and welfare, but this enables him to discuss analytical problems more fully. Yet even he leaves out some questions of importance. He gives a fairly long discussion of import duties, but says nothing about import quotas or exchange control. He discusses taxation at some length without pointing out that if some industries expand and others contract (whether due to taxation or to other causes) relative factor-prices may be substantially changed. Nevertheless if one of these four books had to be chosen as the best I think I should vote for his admirable little treatise.

F. BENHAM.

The Trade Cycle. By R. F. HARROD. Oxford University Press. 10s.

To many readers of ECONOMICA Mr. Harrod's new book will already be familiar. It has been extensively reviewed elsewhere, has attracted the attention of subsequent writers and has been the subject of much discussion in academic circles. In these circumstances a lengthy description of its contents would be superfluous. It seems more appropriate to concentrate on a few outstanding aspects only. Mr. Harrod's approach to his problem is admirable. He starts with the simple question—What governs the level of activity ?—and then, using the well-tried method of successive approximations, proceeds to answer this first for the Crusoe economy. The complications of exchange, employment by entrepreneurs and money are introduced one by one and at each step the author indicates what the " determinants " of activity are and whether they should be regarded as " stabilisers " or " destabilisers." The terms are new but the concepts are familiar. Whether a particular determinant is a stabiliser or not depends on the nature of the functional relationship between it and the amount of activity. Thus the laws of diminishing utility and diminishing returns both exercise a stabilising influence.

Mr. Harrod concludes that in the modern individualist economy the level of activity is determined by four factors—the rates of pay of prime cost factors, the efficiency of these factors, the elasticity of demand and the general price level. The first of these is a " stabiliser " if prime costs are " plastic " and vary directly with output, the second if the law of diminishing returns operates, the third if the elasticity of demand diminishes as output increases. The fourth, on the other hand, is necessarily a destabiliser because—so argues Mr. Harrod—the behaviour of money causes the " general " price level to rise as output increases and to fall as output decreases.

That Plasticity in Prime Costs—providing one abstracts from any influences on the price level—and Diminishing Returns are both stabilisers may be granted. The third and fourth determinants call for more comment.

The diminishing elasticity of demand is regarded by Mr. Harrod not only as the most important stabiliser but also as the chief explanation of that " shift to profit " which plays a large part in the theory of the cycle which he afterwards develops. Conditions of imperfect competition are assumed and it is suggested that as output increases elasticity falls, marginal revenue drops and hence a restrictive influence is exercised. Why should elasticity fall ? Mr. Harrod argues that as people become better off during the recovery, they stop worrying so much about getting the best value for their money, they are governed by habit and less inclined to leave the firms they usually deal with. The Theory is an ingenious one but it is based on very slender evidence. Except for persons whose fortunes fluctuate very greatly indeed, it seems improbable that such fundamental habits change in the short space of a boom or depression. Moreover the important fact is overlooked that during a boom new firms and new products come into existence which would tend to increase rather than diminish the elasticity of demand.

The destabilising influence of the fourth factor—money—would be denied by nobody and is indeed common ground for a number of different theorists. But its expression in " the general level of prices " begs a number of questions—as Mr. Harrod is, of course, aware—and does not carry us very far forward.

Perhaps the most valuable aspect of this first part of the book is the extremely lucid manner in which the author develops his argument from the familiar basis of equilibrium analysis. The style is so clear and the reasoning so logical that even if one disagrees at certain points there is not the slightest difficulty in seeing what the causes of disagreement are. There are unfortunately too many books on the Trade Cycle about which this cannot possibly be said.

The essence of Mr. Harrod's positive theory is contained in the second part of his book. The concept of the general price level is now relegated to the background. In its place emerge the two principles of " The Relation " and " The Multiplier." " The Relation " is the term given by Mr. Harrod to what is more widely known as " The Acceleration principle " : it refers to the peculiar relation between the output of consumption goods and capital goods. This relation is quite simple. Given certain assumptions, an increase in the rate of output of consumption goods requires a far greater proportionate increase in the output of capital goods. If the rate of output of consumption goods is to rise from 2 per cent. to 4 per cent. per annum, then the output of new capital goods—as contrasted with capital goods for replacement—must be doubled. Similarly if there is no increase in the output of consumption goods then no new capital goods are required at all.

On the basis of this principle, Mr. Harrod suggests a complete theory of the trade cycle. Although it is not his final theory—it may be regarded as a kind of reserve or " A " theory—it certainly demands consideration. During the boom the output of new capital goods increases because the unemployed men in the consumption goods trades are available, but the originally unemployed machines have become useless. When, however, the unemployed men in the consumption goods trades have all been taken on again, if there is no mobility of labour, the demand for new capital goods ceases. There is therefore depression in the capital goods trades and this will spread, through the fall in income, to the consumption goods trades as well. This in turn will cause a fall in replacement demands and so depress the capital goods trades still further. Eventually, however, replacement becomes necessary and so the revival begins again.

Mr. Harrod admits that this version really assumes the absence of changes in productive methods or the fixity of technical coefficients between capital and labour in the consumption goods trades. But he takes the view that the removal of this assumption does not make much difference. This seems to be excessively gloomy.

So long as the consumption goods trades continue to be highly profitable there are several reasons why a collapse may be avoided. In the first place some mobility of labour may be anticipated, and even if labour does not move, intermediate products frequently can and do. Secondly, a scarcity of labour is itself likely to lead to an increased proportion of capital in the consumption goods trades. Thirdly, although the rate of interest may be nominally high at this stage in the boom, the prospect for new issues may be still exceedingly good and it is this rather than the rate of interest which will directly affect investment. Fourthly, as the boom continues, if obsolescence is fairly rapid, demands for replacement will increase too.

The final version of Mr. Harrod's theory involves a combination of this principle of "The Relation" with the Keynesian doctrine of "The Multiplier." The increase in net investment gives rise to an increase of income and output sufficient to make savings equal investment. The increase in income will not, however, be proportionate to the increase in investment, because of the shift to profit (explained by the diminishing elasticity of demand) and the propensity to save more from a larger income. These two factors—the shift to profit and the propensity to save—together with "the amount of capital" are known as "The Dynamic Determinants." Together they determine the extent to which new investment will continue. The first two tend to restrict it, the third to encourage it. So long as they counterbalance one another, the increased rate of consumption will be just sufficient to absorb the output of new investment goods.

A point will be reached, however, when the influence of the first two becomes predominant. At that point the new capital equipment will be greater than is required by the demand for consumption goods and the technical conditions. The rate of increase of investment must decline. This in turn will cause a fall in the rate of increase of income and consumption. "This, in accordance with the Multiplier, entails an absolute fall in income and consumption. This, in accordance with the Relation, entails that net investment is rapidly reduced to a very low level, if not to zero."

Anything like a thorough analysis and discussion of this theory is clearly far beyond the scope of a short review. Few unprejudiced readers would deny that Mr. Harrod has thrown much additional light on what is at once the most complex and the most vital of all economic problems to-day. Even if one disagrees at certain points with the emphasis he places on "The Relation," there can really be no doubt that it is a phenomenon which no serious analysis can possibly omit. The introduction of imperfect competition is also a very valuable step forward. The point which is likely to give rise to more dispute is the importance attached to the propensity to save and the shift to profit. Here surely the time sequence must not be overlooked. As the boom continues the increase in real as contrasted with money income must diminish. If this is so the dangers inherent in the propensity to save must diminish as well. Thus the explanation of a collapse after full employment is reached and has been maintained must lie—according to Mr. Harrod—entirely with the shift to profit. It seems more likely that in such circumstances a combination of psychological and purely monetary factors is responsible. One is driven—perhaps by the very completeness of such a theory as Mr. Harrod's—to a certain scepticism and to the wholly unoriginal reflection that it is probably useless to look for a single explanation for the collapse of every boom, although throughout the course of every cycle similar forces are clearly at work.

In the last part of his book Mr. Harrod turns to the question of remedies. With very becoming but rather unnecessary modesty he describes this as only presenting " matter for reflection." It is to be hoped that, in spite of the difficulties, he will be able to proceed further with this part of his work. For there is much to be said for the view that those differences which still exist among economists about the nature of the trade cycle would become clearer if the problem were now to be approached from the angle of policy. And that clarity even about differences is the way to discovering truth would be denied only by those whose faith in the social sciences is really non-existent.

H. T. N. GAITSKELL.

The Framework of the Pricing System. By E. H. PHELPS BROWN. Chapman & Hall. 1936. xvi + 221 pp. 10s. 6d.

This book is written for students who, having done some introductory reading, " are now in a position to begin a systematic study of theory ". It is not, however, addressed to all who are in this position ; only to those students who come without a knowledge of calculus. The mathematical economists they cannot read. The literary economists, Mr. Brown thinks, concerned with the relation of theory and practice, do not expose sufficiently clearly the framework of theory on which they are building. Consequently, many students who rely on them fail to perceive the unity and interdependence of the economic system. It is for such students that Mr. Brown writes.

Mr. Brown treats his subject by considering the consumers' and producers' markets separately, finally connecting up the two to form a unified system. This he does without using any mathematics other than the ordinary school algebra, and he succeeds brilliantly in enabling such a student " to grasp the unity of the pricing system, which it has been the achievement of mathematical studies to make clear, but which literary exposition fails so often to enforce ". On one point, however, students may need warning. Mr. Brown when discussing the principles governing the distribution of a consumer's income between different products, avoids the use of marginal utility and replaces it by what he calls the "marginal rate of substitution". His treatment is, however, in some ways much more akin to the ordinary marginal utility approach than it is to that recently brought to our attention by Messrs. Hicks and Allen. Mr. Brown constructs his substitution function by considering the change in the marginal rate of substitution when one commodity is increased, the other being assumed to remain the same. This is different from the increasing marginal rate of substitution of Messrs. Hicks and Allen which is concerned with movements along a single indifference curve. Indeed, as they point out, there is no necessary reason why the marginal rate of substitution should diminish in the way Mr. Brown assumes in his examples. Students who have read Mr. Brown's book and who then turn to this recent exposition of the theory of value may therefore be confused.

Although Mr. Brown succeeds in his object, it is none the less, as he himself points out, a difficult book for the student to read. For a certain type of student it is admirable. But one wonders whether there are many students who will read it with the care essential if it is to be understood.

There is a misprint on page 56. "6 min. 22 sec." should read "6 min. 2 sec."

R. H. Coase.

The Mathematical Problem of the Price Index. By J. Montgomery.
P. S. King & Son, Ltd. 1937. 74 pp. 6s.

This clumsy book should have been a snappy mathematical note. The author is concerned to define a price index and a quantity index for a group of commodities. "The price index must correspond to the change in the aggregate value due to changes in price, and the quantity index must correspond to the change in the aggregate value due to the change in quantity" (p. 3). A set of mathematical assumptions (scattered through pages 5 to 28) provide a meaning for the vague words "correspond to"; in effect, the author divides up each price-change and quantity-change for each commodity into small equivalent units and postulates that to each unit "corresponds" the same change in the logarithm of the Total Value. From these assumptions we are led at length to a pair of terrifying formulæ for obtaining the required indices (p. 37). Unfortunately the arithmetical work required for extracting the indices from any actual set of data would extend to 18 columns (p. 67).

It is hinted (p. 51) that the author's assumptions are in some way preferable to the many alternative sets available: since they are couched in mathematical symbols, it is very hard to interpret them, but the following analogy is at any rate clear. Having divided the time-interval (during which prices and quantities change) into many elementary portions, and compiling price- and quantity-indices by the chain method, we should arrive at the author's formulæ, *if we assumed that each price and each quantity had increased at a uniform geometric rate during the interval.* There is no obvious reason for selecting this particular assumption or the author's equivalent (tacit) mathematical postulates.

A more manageable formula[1] for the indices can be obtained from use of the above chain-method, if we assume a *constant arithmetic* rate of increase for each price and each quantity during the interval: Mr. Montgomery could have written as useful a book about this formula, since by using it the required indices can in a few minutes be worked out, if we are given the ordinary price- quantity- and value-indices for the group, constructed from base-year weights.

D. G. CHAMPERNOWNE.

Milk Consumption. By K. A. H. MURRAY. Oxford University. Agricultural Economics Research. 1937. 64 pp. 2s. 6d.

Modestly, Dr. Murray gives the opinions of other experts first. A League of Nations' Committee on the Problem of Nutrition says " milk is the nearest approach we possess to a perfect and complete food, and no other single food is known that can be used as a substitute." Our own Advisory Committee on Nutrition says " the consumption of a sufficient quantity (of milk) may be regarded as the key to proper nutrition." The members of these committees must be enthusiasts, and enthusiasts tend to overstress the importance of their subjects. Dr. Murray points out that the important food elements, calcium and animal proteins, together with vitamins A and D, may be obtained much more cheaply than by consuming fresh milk. " A gallon of milk as cheese and butter, with only the value of the whey missing, costs about 7½d., whereas a gallon of fresh milk costs 24d." The reasons given for increasing the consumption of milk tend to be overemphasized. " So far as health is concerned it is probable that the bulk of the alleged deficiencies in diets, both in quantity and in quality, could be remedied, and will probably be remedied by foods cheaper than liquid milk; the qualities of milk as a food for children, however, would make a special claim for more milk for them." After this it is surely strange to suggest that the public be persuaded to purchase more of this dear food by means

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[1] Let us construct price- quantity- and value-indices p q v upon base-year weights for the group of commodities. Then the required price and quantity indices P and Q can be obtained from p, q and v, by use of the following formula:

Assuming all indices to stand at 1.00 in the base-year, and that the symbols p, q, v, P and Q refer to their values in the later year,

$$P^2 = v\left(\frac{m+k}{m-k}\right)^{\frac{n}{k}} \qquad Q^2 = v\left(\frac{m+k}{m-k}\right)^{-\frac{n}{k}}$$

$$\text{where } m = \tfrac{1}{2}(p+q), \quad n = \tfrac{1}{2}(p-q), \quad k^2 = m^2 - v.$$

Not having studied the literature of the subject, I cannot say whether this formula is a new one: it is however convenient because the basic elements p, q and v are often to be found already worked out in statistical publications.

of extensive advertising, and it seems libel to say—" possibly the biggest obstacles (in the way of increasing the consumption of milk) are antipathy, indifference and prejudice ". These words " antipathy, indifference and prejudice" have been widely quoted by newspapers, and even by a representative of the Government in the House of Commons.

The introduction summarises ably, but sometimes a little carelessly, the work of others investigating milk consumption. These investigations show that milk consumption increases with income and that *per capita* consumption is low in large families, which usually means families with children. Dr. Murray's special contribution is a valuable survey of milk consumption in Oxford obtained by enquiry at every thirty-fifth house. Information from 587 households was obtained. The survey shows Oxford people to be big milk consumers, averaging 3.94 pints per week, whilst the Co-operative Society distributes 39 per cent. of the milk, leaving the remainder to some 120 other dairymen. In families without children consumption averaged 4.6 pints per head; in families with three children only 2.7 pints per head per week. Milk consumption increases rapidly with expenditure on food when that expenditure is low, more slowly when it is high. A linear multiple regression equation is applied over the whole range of the sample, including the 25 per cent. of households which spend less than 16 shillings and the 25 per cent. which spend above 43 shillings per week on food. As a result it would appear that an extra shilling spent on food will only increase milk consumption by ¼ pint, whereas the actual increase in the 80 per cent. of the households spending less than 15 shillings per head on food is much greater than this.

The most interesting section of the work is a pioneer effort attempting to identify other factors besides income and family composition which affect milk consumption. Dr. Murray is very impressed by the fact that people's tastes differ, that some families will buy more milk than others with a similar expenditure on food and similar composition. Quite rightly he seeks reasons for this by asking the householders the purposes for which they use their milk. Unfortunately the householders seem to have guessed the answers to these questions, and the " proportions showed no consistent trends " and rarely any trends at all. About 33 per cent. was used in tea, coffee and cocoa and 40 per cent. in cooking as expenditure increased from 5 to 30 shillings per head, whilst lack of change is also noticeable as the number of persons per household increased from two to eight. About the only features which look significant are that a bigger proportion is drunk raw in families with children, and a bigger proportion is consumed in tea, etc., in poor families and in families without children. But it should be explained that " in tea, coffee, cocoa " really means " not otherwise accounted for ". The proportion used in milk drinks such as Ovaltine and Bournvita increases with expenditure on food.

H. S. Booker.

The Twilight of American Capitalism. By A. S. J. BASTER. With an introduction by Professor VON MISES. P. S. King & Son, Ltd. 1937. 216 pp. 9s.

Public opinion in democratic countries is now generally convinced that partial economic planning in the form of self-government by groups of producers can not only solve the economic problems but also prevent the degeneration of political democracy into some form of totalitarianism. In an attempt to inform public opinion on this question Dr. Baster has made an important contribution to the literature on planning. He examines critically, though with sympathy for the ideals of the reformers, the economic and political implications of the New Deal as an experiment in partial planning.

Dr. Baster brings out very clearly the extreme vagueness of the planners' aims and the general inability to understand the nature of the economic problem. His excellent discussion of the views on "wasteful exploitation" of the National Resources Board shows how completely the planners failed to distinguish between economic and technical problems. Where the aims were more precise, as in the A.A.A., Dr. Baster shows that this precision was illusory and meaningless. The main disappointment in the New Deal, however, was that intervention in industry and agriculture took the usual form of monopolistic restriction and the ossification of the economic system by measures which guaranteed to existing producing interests the maintenance of profits and incomes by restrictions on those readjustments made necessary by changes in demand and technique. The experience of N.R.A. constitutes an excellent refutation of the belief that if all interests are represented on a board with powers to control production and prices the public interest will be safeguarded—what Dr. Baster calls "one more instance of the fatal obsession of the Anglo-Saxon mind with the pathetic mythology of the committee table". N.R.A. was little more than a bribe to business men to support the shorter hours and higher wages policies of the Administration—the cost to be borne by the consumer. As an example of long-run planning N.R.A. was almost everything it should not have been. As a recovery measure it probably neutralised to a very large extent any good there was in other recovery measures.

The New Deal was concerned with both recovery and reform. Dr. Baster makes it clear that some large-scale intervention by the Government was politically inevitable in 1933 in order that the system should be saved from collapse. The magnitude of bank failures and of the general liquidation of the abnormal debt structure made something like the Reconstruction Finance Corporation necessary as an emergency measure. "But there was available no criterion whatever for limiting the Corporation's advances to any particular business or line of business, no definitive reason for believing that the personal opinions of its directors in this matter would be more reliable in the long run than the collective judgment of business men in a market".

Dr. Baster approves of the provisions whereby more information should be made available to investors through the Securities and Exchange Commission, though he is aware of the possibilities of evasion. He also approves of the power which the Federal Reserve Board now has to change the reserve proportions of member banks; but he is very critical of the remainder of the banking reforms.

Dr. Baster's discussion of the T.V.A., though short, raises all the important issues. The whole question of the possibility of simulating competition in Public Utility Regulation is put to the test in T.V.A. He shows that the "yard-stick" by which private monopolies are to be measured cannot determine "competitive" rates and charges. The impossibility in practice of separating the costs of flood control and improved navigation makes any figure of the costs of generating electricity purely arbitrary. As a test case of economic planning T.V.A. has to be rejected.

In the last two chapters of the book, entitled "The Political Background" and "Implications", he raises what must appear to many to be the fundamental issue in any discussion of planning. He analyses partial planning as a problem not of economics but of political economy. Advocates of partial planning usually imagine themselves to be defending political democracy. In Dr. Baster's view, however, "it is questionable whether the connection of democracy with the competitive system is such an accident as the interventionists would have us believe". The main thesis of the book is that a "freely competitive system is the best guarantee for effective democracy that has yet been devised". The growth of monopolies of labour and capital under statutory protection results in the substitution of economic conflicts between different economic groups, on a scale so large that the Government is forced to intervene on one side, for the impersonal and determinate distribution of the national dividend brought about under competitive institutions. The failure to prevent the rise of monopolies and rigidities in the economic system, which was due very largely to bad legislation and the Supreme Court's interpretation of the anti-trust laws, was, as Dr. Baster shows, mainly responsible for the renunciation of competition by public opinion and its acceptance of policies which made it more and more difficult for the Government to remain impartial between different groups "each with its particular quota of persons rich and poor, firms successful and unsuccessful, efficient and inefficient". The tragedy of the New Deal lies in the inability of people on the whole hostile to authoritarianism to understand not only the political implications of partial planning but also that if they had failed to maintain the competitive system they were even less likely to solve the immeasurably greater difficulties involved in any form of planning designed to give people what they want.

R. F. Fowler.

Retail Distribution. By Henry Smith. Oxford University Press. 1937. 175 pp. 7s. 6d.

Having regard to the paucity of studies of retail distribution by economists one may be grateful for an essay which " applies the tools of economic analysis to the problems of retail distribution in an attempt to detect, determine, and evaluate the forces which are at work ". Mr. Smith shows very clearly the extremely heterogeneous nature of the product of retailing and the growing importance of retailing services other than the primary one of economising in the holding of stocks. He analyses with skill the forces determining the structure of retail prices in a perfect market and the modifications which have to be made when account is taken of factors preventing such market perfection. Of special interest to students of business administration will be his discussion of a few of the problems which have to be solved by the department store—problems of pricing resulting from the difficulties involved in the determination of separate costs for each type of goods and the possibility of estimating more accurately the demand curves for different goods through customer classification.

The main object of the book, however, appears to be to explain the increase in the so-called " cost of retailing ". Mr. Smith has little difficulty in showing the emptiness of most of the popular explanations ; but his evident enthusiasm for recent work in the theory of imperfect competition has led him to some extremely questionable interpretations of the scanty available statistics of retail trade in an endeavour to prove that " the dominant factors in the growth of retail costs have been the imperfection of the retail market ". Mr. Smith presents us with some new and interesting estimates of the rate of growth of different types of shop in certain areas. He is impressed by the fact that the rate of growth of shops during the period 1921–1931 exceeds in most areas the rate of growth of population and thinks that this cannot be explained by reference to migration. In the interpretation of these figures, however, Mr. Smith seems to have overlooked the importance of the lessening density of population in large cities such as London, a factor which would increase the cost of a given degree of availability of shops—as he points out in an earlier chapter.

According to Mr. Smith's estimates, the total gross margin or " cost of retailing " increased from £362 Mn. (16 per cent. of the retail value) in 1924 to £586 Mn. (25 per cent. of the retail value) in 1931. He argues that as wholesale prices fell " retailers in general tended to let retail prices lag behind them and to spend their increasing margins upon attaching customers more firmly to them by means of providing more and more expensive services. . . . Therefore the retail market has been steadily becoming more and more imperfect " (p. 130). This is surely an illogical inference. Mr. Smith assumes, without offering any evidence, that consumers would have preferred lower prices to these extra services which they have, in fact, obtained. His error is all the more surprising in view of his careful explanation in Chapter I of the very diverse nature of the demand for retailing services—an explanation which should make anyone doubt the possibility of inferring anything about the efficiency of retailing from such aggregate figures. It is true that as people move from densely populated areas to less densely populated areas a greater proportion of consumers will be attached to shops where the elasticity of demand was formerly below the average. But this process of diffusion must increase the total costs of distribution whatever the elasticity. It has yet to be proved that costs have been unnecessarily inflated as population became less concentrated.

The whole case for the applicability of the theory of "excess capacity" to retailing depends, according to Mr. Smith, on the growing imperfection of the market when the number of retailers increases. Yet he offers no direct evidence that this surprising result does, *in fact*, happen. If he had confined himself to pointing out that the increase in the quantity of goods sold subject to price maintenance had been a cause of inflated costs his conclusion would have been acceptable. The ease with which price cutting can develop when price maintenance is not rigidly enforced should make anyone sceptical of the possibility of "excess capacity" *on a wide scale* in the absence of price regulation. The theory of "excess capacity", like the "cobweb theorem", must be regarded as little more than an interesting *curiosum* with limited applicability and importance until considerably more evidence is offered that the necessary assumptions do, in fact, describe the behaviour of the majority of producers in a particular market.

Nevertheless, in spite of the insecurity of his conclusions on this question, Mr. Smith has written a very useful and stimulating book which will be welcomed by all those interested in problems of retail distribution.

R. F. Fowler.

Recent Important Foreign Books on Economics

The Librarian of the British Library of Political and Economic Science has made arrangements, as a supplementary check to the purchases of the Library, to receive regularly every quarter from competent foreign scholars lists of the most important books in their respective fields of study published in their own countries (or languages as the case may be) during the preceding three months. It appeared to the Editorial Board of ECONOMICA that these lists might prove useful to other institutions, and the lists of books on economics will therefore be published regularly in future issues of ECONOMICA. It must be understood, however, that the Editorial Board of ECONOMICA accepts no responsibility for the selection of the books included in the list.

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(Selected by Professor H. Gideonse).

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